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Essays in Labor Economics: Evidence from Danish Micro Data

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Essays in Labor Economics

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Evidence from Danish Micro Data

Esben Anton Schultz

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PhD Thesis

Copenhagen Business School and CEBR

November 2011

Esben Anton Schultz
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Preface

The vast majority of my PhD thesis was written in the period from September 2006 to February 2010 while I was a PhD student at Centre for Economic and Business Research (CEBR) and the Department of Economics at Copenhagen Business School. The final editing was done in November 2011. I am grateful to the Tuborg Foundation for funding my stipend.

This thesis would not have been possible without the tremendous support and encouragement I have received from family, friends and colleagues throughout my PhD studies. A few deserve more than a general word of thanks. First of all, I am grateful to my supervisor Anders Sørensen for his invaluable advice and guidance throughout my PhD studies. Hans Christian Kongsted also deserves thanks for being my co-supervisor. I also want to thank my co-authors: Carl-Johan Dalgaard, Henrik Jacobsen Kleven, Camille Landais, Emmanuel Saez and Anders Sørensen. It has really been inspiring to work with all of you.

Furthermore, I thank my former colleagues at CEBR and the Department of Economics at CBS for making these years enjoyable, and I owe a special thanks to Peter Møllgaard and Svend-Erik Hougaard Jensen not only for creating an outstanding research environment, but also for always taking an interest in me. In particular, I am indebted to Frederik Vinten, Christian Scheuer and Battista Severgnini for many helpful comments, discussions and laughs.

During my PhD I spent three months at the Department of Economics at London School of Economics (LSE), a stay from which I have benefited tremendously. Especially I want to thank Henrik Jacobsen Kleven. Moreover, my visit at LSE would not have been possible without the generous contributions I received from Arbejdsgiverforeningens Jubilæumsfond, Augustinus Fonden, Hedorf Fond, Jørgen Buchs Fond, Konsul Axel Nielsens Mindelegat, Oticon Fonden and Otto Mønstedts Fond.

Throughout my PhD I have found presenting my work in numerous workshops and conferences to be extremely beneficial. I would like to thank CEBR and ECON for making this financially possible. I am also very grateful to the Centre for Applied

Microeconometrics at the University of Copenhagen for providing me with data for the third chapter of this thesis.

Very special thanks to my closest family and friends for always supporting me. To my wonderful children, Ava and Louie, for always giving me a reason to smile and reminding me that life is much more than just work. Last, but certainly not least, I would like to thank my girlfriend Kathrine for her encouragement, patience and unconditional support even in situations where I could not expect it of anyone. I could not have done it without you!

Esben Anton Schultz
Copenhagen, November 2011

Introduction

This thesis consists of four empirical essays within the broad field of modern labor economics. All four essays are self-contained and can be read independently of the others. Chapter 1 investigates the distinct effects on information technology and communication technology on firm's skill demand. Chapter 2 studies whether the observed wage gap between majors in human arts and other fields are caused by their education per se or by selection. Chapter 3 examines taxable income responses to variation in marginal tax rates. Chapter 4 analyzes the effect of income taxation on the international migration of top earners. Each chapter provides an independent and separate analytical contribution to their specific field. A detailed description of each chapter's contribution to the existing literature is not included here. For that, I refer to the chapters themselves.

Although both Chapter 3 and Chapter 4 studies behavioral responses to individual taxation, the four chapters are not closely related by topic. However, they all share one key feature. That is, the empirical analysis in all chapters is based on Danish micro data. These data are administered and maintained by Statistics Denmark that has gathered the data from a number administrative registers, including the Integrated Database for Labor Market Research (IDA), the Income Tax Registry and the Danish Student Registry. To facilitate register-based research Statistics Denmark provides access to these data to researchers and analysts in authorized environments. The data set runs from 1980 to 2009 and covers the entire population of individuals in Denmark. For each individual, the data set contains complete data on educational and labor market histories along with detailed information on a large set of socio-economic variables as well as comprehensive income and tax return information. From 1995 onwards, detailed information for all firms in Denmark is also available. These data can be linked to individuals to form a matched employer-employee data set.

Below I describe the four chapters in further detail, emphasizing how the Danish micro data along with the institutional setting and sharp policy reforms combine to make Denmark a very useful laboratory for studying the issues at hand in this thesis. By doing so, I also hope that it will become clear that the chapters are also interrelated in terms of empirical methodology.

In Chapter 1, I propose the hypothesis that firms' use of information technology (IT) leads to skill-upgrading, while their use of communication technology (CT) leads to skill-downgrading. This is in contrast to existing literature on the skill content of technological changes that typically assumes that all types of ICT have the same impact on firm's demand for human capital.

I empirically test the hypothesis using a panel data set of more than 6,000 Danish firms over the years 2002-2008 that merges the administrative matched employer-employee data set with ICT expenditure surveys that have also been collected by Statistics Denmark. These data combine three key advantages relative to the data that have been used in previous studies of the impact of ICT on skill demand. First, the data contain detailed information on skill characteristics and wages for each worker at a given firm. Second, the data contain disaggregated ICT expenditures on various subcomponents of ICT at the firm level, which allow me to distinguish between IT and CT. Finally, these data allows me to follow firms' use of IT, CT and skills over time.

As no obvious natural experiment exists, I rely on differential changes in the quality-adjusted prices of IT and CT to identify the effects of IT and CT on skill demand. The idea behind this empirical strategy is that differential changes in the quality-adjusted prices of IT and CT, which have been far from negligible over the past couple of decades, create different incentives for firms to adopt IT and CT. Estimating a translog cost function using ordinary least squares (OLS), I provide empirical evidence that confirms the hypothesis. Nevertheless, a potential problem with the empirical strategy is that omitted variables and measurement error may still explain some of the correlations I find. To try to make additional headway, I employ an instrumental variable (IV) approach to the issue at hand. Specifically, I use industry-level measures of IT and CT as instruments for firm-level IT and CT. However, employing this IV strategy does not to alter the simple OLS results.

Although I view this analysis as a useful first step given the lack of evidence of this issue, there are many extensions in this line of research that could be pursued. First, additional empirical work analyzing natural experiments is needed to more carefully identify the causal effects of IT and CT on skill demand. One could perhaps utilize the

liberalization/deregulation of the telecommunication industry that has taken place over the last decades in Denmark and many other countries as natural experiments, generating exogenous differences in the effective price of telecommunication over time and across countries. Second, an important area for future research is to study the distinct effects of different types of ICT on not only skill demand but also productivity, firm organization and wage inequality.

In Chapter 2, we study whether the observed wage gap between majors in human arts and other fields are caused by their education per se or by selection, using the Danish micro data. OLS estimates for Denmark suggests that the raw wage gap between human arts majors and other majors is about 20%. The Danish educational system is particularly well suited for studying the returns to different types of education. The reason is that university degrees in Denmark are highly specialized. For example, if one chooses to study economics then this is the subject matter pursued throughout the entire time at the university; both during undergraduate and the graduate level. Intellectual excursions into other fields only occur to a very modest extent, in contrast to what may be the case under e.g. a US-type system. Consequently, examining the labor market performance of Danes holding different types of tertiary education is likely to convey information about the extent of human capital production within different fields of study. In addition, Danish universities are publicly funded which reduces the scope for marked quality differences.

Accordingly, we attempt to elicit information about the causal effect of *the field of study* on individual productivity, as it manifests itself in individual wages. OLS estimates are unlikely to capture the causal effect of the type of education on individual productivity, unless relative cognitive abilities are controlled for. Accordingly, we try to control for comparative intellectual advantages by invoking individual-level information about academic specialization in the Danish high school system, high school attended, high school GPA, and parental education. Still, a significant wage difference between human arts majors and other majors persists. Ultimately, it is hard to rule out that other – unobserved – factors could simultaneously impact on the choice of education as well as productivity. As in Chapter 1, we therefore employ an IV approach. We document that

individuals' educational choice is correlated with that of older students in high school, suggestive of peer effects. Conditional on high school fixed effects, this link is unlikely to affect post-university wage and thereby constitutes a plausible instrument for the educational choice. With this instrument in hand, we proceed to estimate the impact of choosing an education in the humanities by 2SLS. Our 2SLS estimates differ substantially from the conventional OLS counterparts. After instrumenting the wage difference between human arts majors and other majors disappears. Hence, we are led to the following conclusion: the relative poor performance of human arts majors in the Danish labor market is mainly due to selection rather than to low human capital production at universities.

Our analysis raises new questions worth exploring in future research. First, wage differences seem to be related to relative cognitive abilities; mathematics appears to be important, for example. But why is that? Is it because such abilities are relatively scarce in the population or because they are particularly productive? If the latter is the case, then it would be useful to try and discern why such abilities are in high demand. Second, how are relative abilities in, e.g., math and human science formed? as they determine both educational choice and wages, it would be useful to know whether these cognitive traits have a genetic origin, or are acquired during primary and secondary schooling. If the former is the case, education policies cannot be invoked to influence them; end conversely if relative talents are acquired.

In Chapter 3, we study taxable income responses using administrative data that link tax return information to detailed socioeconomic information for the entire Danish population over the years 1984-2005. The identifying variation is provided by a series of Danish tax reforms – the 1987, 1994, 1999 and 2004 reforms – that create substantial tax variation across individuals, income forms, and over time. The identifying variation provided by the Danish tax reforms does not feature the same strong correlation between tax changes and income levels as the tax reforms in the U.S. (and other countries) that have been extensively studied in the past two decades. Arguably, this allows us to overcome the identification problems arising from non-tax changes in the income distribution and mean-reversion that plague much of the existing literature.

To estimate the elasticity of taxable income with respect to the net-of-tax, we use a difference-in-differences approach that relates changes in taxable income over time to changes in marginal tax rates over time for individual taxpayers. Because of the nonlinearity of the Danish tax system, the marginal tax rate is endogenous to the choice of taxable income. We therefore construct instruments for the marginal tax rate using mechanical tax changes driven only by changes in tax laws. Subsequently, we estimate our model using 2SLS, as in Chapter 1 and Chapter 2.

Our main findings are the following. First, labor income elasticities are modest seen over the full time period, around 0.05 for wage earners and 0.10 for self-employed individuals. Second, capital income elasticities are about 2-3 times larger than labor income elasticities. Third, behavioral responses (both labor and capital) are substantially larger when estimated from large tax reform episodes versus small tax reform episodes. This finding is consistent with the argument that elasticities estimated from small tax changes are attenuated by optimization frictions (e.g. adjustment cost and inattention), whereas large tax changes are likely to overcome such frictions and hence reveal the true long-run elasticity. We find that a particularly large and salient tax reform in the late 1980s is associated with a population wide elasticity of labor income of about 0.12, whereas a series of smaller tax reforms in the 1990s and 2000s are associated with a labor income elasticity of only 0.02. Fourth, cross-tax effects between labor and capital income are in general weak, with a small degree of complementarity between the two income forms for wage earners and a small degree of substitutability for self-employed individuals. Since income shifting for tax avoidance purposes by itself would imply substitutability, our results suggest that income shifting is more prevalent for the self-employed than for wage earners.

In Chapter 4, we estimate the effect of taxation on international migration of top earners. As in Chapter 3, the identifying variation is generated by Danish tax policy changes. Specifically, we use the Danish Tax Scheme for Foreign Researchers and Key Employees (*Forskerordningen*) – the Researchers’ Tax Scheme for short – which offers preferential tax treatment to foreign scientist and high-income foreigners in other professions working in Denmark. This scheme, which started on June 1, 1991, allows foreigners (and in some

cases Danes, who have been abroad) with incomes above a certain threshold (about 103,000 Euros as of 2009) to be taxed at a flat rate of 25% for a total period of up to 3 years instead of the regular progressive schedule, where workers with earnings above the scheme threshold would face average income tax rates around 55%. The earnings eligibility threshold does not apply to researchers.

This unusual piece of tax policy implies large discontinuities in tax liabilities depending on the contract start date (before and after June 1, 1991), duration of stay in Denmark (3-year rules), earnings level (earnings eligibility threshold), and occupation (researchers versus other professions). Hence, the reform creates very large quasi-experimental variation along several different dimensions, and provides a very powerful way of identifying the effect of taxation on migration. It is very unusual to have large discontinuities in tax liability (as opposed to discontinuities in marginal tax rates) in tax systems. We explore the different aspects of the tax scheme using quasi-experimental techniques such as bunching approaches and difference-in-differences. For this analysis, we have access to the *standard* administrative data for the entire population of Danish residents (Danish citizens and foreigners) since 1980. In addition, the data set have been augmented with variables relating specifically to the Researchers' Tax Scheme, which the Danish tax authorities (*SKAT*) have made available specifically to this project.

Our analysis of the Danish Researchers' Tax Scheme for foreigners yields two main results. First and most important, we obtain compelling evidence that the scheme have almost doubled the number of highly paid foreigners relative to slightly less paid ineligible foreigners, which translates into a very large elasticity of migration with respect to the net-of-tax rate in excess of one. Second, we show that there is bunching of earnings at the scheme eligibility threshold and bunching of durations of stay at the three year duration limit. In the end, the migration elasticity is much larger than the conventional within country elasticity of reported earnings found in the literature in general as well as in Chapter 3. Hence, preferential tax schemes for highly paid workers could generate very harmful tax competition across European countries and severely limit the ability of European governments to use progressive taxation. This will require international

coordination and the design of rules regulating such schemes particularly in the European Union.

Another important rationale put forward by the Danish tax authorities as well as other European governments which have adopted similar preferential tax scheme for highly paid foreigners is that highly skilled workers generate positive externalities on their co-workers and the economy at large. Therefore, in future work, we hope to make progress on estimating spillovers of scheme-workers on co-workers and broader economic growth in two ways. First, we could use a Bartik type instrument measure for the exposure of each industry or firm to the scheme. The instrument would be the fraction of highly paid foreign workers in the industry or individual firm just before the introduction of the scheme. This would allow us to measure the effect of attracting scheme workers on wage dynamics of domestic workers or non eligible foreigners as well as firms' hiring and growth behavior. Second, we plan to study specifically the researcher part of the scheme that we have not analyzed in this paper. We plan to analyze the effect of the influx of researchers in Denmark on the patents, publications, and placement of Ph.D. candidates of Danish research centers vs. comparable European countries such as Sweden. The comparison could be done across academic fields using the pre-scheme fraction of foreign researchers across fields. To carry out these analyses, our data set would have to be augmented with the administrative matched employer-employee data set, which is used in Chapter 1.

Summary

Chapter 1: Not All Technologies Are Alike: Distinguishing the Effects of Information Technology and Communication Technology on Skill Demand

Existing empirical studies on the skill content of technological changes typically assumes that all types of information and communication technology (ICT) have the same impact on firm's demand for human capital.

In this chapter, I propose and test the alternative hypothesis that information technology (IT) complements high-skilled labor, while communication technology (CT) substitutes high-skilled labor. The reasoning underlying this hypothesis is straightforward. Through improved access to information, IT will tend to increase the skill requirements of workers, as they have to handle more information and solve more of the problems they face without relying on others. Conversely, by facilitating communication, CT will tend to reduce the skill requirements of workers, as they can specialize further and rely more on others when solving problems.

I empirically test the hypothesis using a panel data set of Danish firms over the years 2002-2008 that merge an administrative matched employer-employee data set with ICT expenditure surveys. The empirical analysis provides evidence that support my hypothesis. Using a translog cost function framework, I document that increases in IT increase the relative demand for high-skilled labor, while increases in CT reduce the relative demand for high-skilled labor. I also show that employing instruments for IT and CT, considering alternative specifications and samples, and controlling for potential confounding factors does not seem to alter this result. Furthermore, I show that the demand for skills is being driven by changes in both employment and wages. Moreover, I find that these effects are non-trivial: IT account for about 24 percent of the change in skill demand, while CT account for about – 14 percent. It thus seems that IT and CT are indeed heterogeneous technologies in relation to skill demand. Moreover, the analysis suggests that failing to account for the differential impact of IT and CT can potentially be a serious error.

Chapter 2: Do Human Arts Really Offer a Lower Return to Education? (Co-authored with Carl-Johan Dalgaard and Anders Sørensen)

In this chapter, we study whether the observed wage gap between majors in human arts and other majors are caused by education per se. Baseline OLS regressions reveal that students of human art fare the worst in the Danish labor market with an hourly wage rate about 20 percent below that of graduates within other majors.

However, one may suspect that the partial correlation between the type of education and wages does not convey accurate information about human capital production. If the selection into educational types is non-random, the OLS estimates will be biased. Our analysis confirms that selection seems to be at work. Socioeconomic circumstances, absolute ability (measured by high school GPA) and relative cognitive abilities (measured by high school course work) influence the choice of education type.

Consequently, we invoke instruments for education type to address the selection problem. We document that individuals' educational choice is correlated with that of older students in high school, suggestive of peer effects. Conditional on high school fixed effects, this link is unlikely to affect post-university wages and thereby constitutes a plausible instrument for the educational choice. Strikingly, once education type is instrumented, we find no significant difference in the wage gap between human arts majors and other majors. This result suggests that a tertiary degree in humanities do not provide individuals with significantly less productive human capital than other types of tertiary education. Accordingly, the relatively poor performance of human arts majors in the Danish labor market is mainly due to selection according to relative cognitive abilities, rather than to low human capital production at universities.

Chapter 3: Estimating Taxable Income Responses Using Danish Tax Reforms (Co-authored with Henrik Jacobsen Kleven)

In this chapter, we present evidence on taxable income responses, using administrative data that link tax return information to detailed socioeconomic information for the entire Danish population over 22 years. The identifying variation is provided by a series of tax reforms that provide substantial tax variation across individuals, income forms, and over time. The

identifying variation provided by the Danish tax reforms does not feature the same strong correlation between tax changes and income levels as the tax reforms in the U.S. and many other countries that have been extensively studied in the past two decades. Arguably, this allows us to overcome the identification problems arising from non-tax changes in the income distribution and mean-reversion that plague much of the existing literature. Unlike previous studies, our results are very robust to the specification of controls for non-tax changes in the income distribution and mean-reversion, which suggests that we have controlled in a sufficiently rich way for non-tax factors impacting on reported income. The Danish setting therefore offers a useful laboratory for a credible identification of taxable income responses.

Our main findings are the following. First, labor income elasticities are modest seen over the full period, around 0.05 for wage earners and 0.10 for self-employed individuals. Second, capital income elasticities are about 2-3 times larger than labor income elasticities. Third, behavioral elasticities (both labor and capital) are substantially larger when estimated from large tax reform episodes than for small tax reform episodes. This is consistent with the idea that responses to small tax changes are attenuated by optimization frictions such as adjustment costs and inattention, whereas large tax changes are likely to overcome such frictions and hence reveal the true long-run elasticity. Fourth, cross-tax effects between labor and capital income are in general weak, but suggests that income shifting is more prevalent for the self-employed than for wage earners.

Chapter 4: Taxation and International Migration of Top Earners: Evidence from the Foreigner Tax Scheme in Denmark (*co-authored with Henrik Jacobsen Kleven, Camille Landaïs and Emmanuel Saez*)

In this chapter, we analyze the effects of income taxation on the international migration of top earners using the Danish preferential foreigner tax scheme. This scheme, introduced in 1991, allows immigrants with high earnings (above 103,000 Euros per year as of 2009) to be taxed at a flat rate of 25% for a duration of three years instead of the regular progressive schedule with a top marginal tax rate of 59%. Using population wide Danish administrative tax data, we show that the scheme doubled the number of highly paid foreigners in

Denmark relative to slightly less paid ineligible foreigners, which translates into a very large elasticity of migration with respect to net-of-tax rate in excess of one. There is bunching of earnings at the scheme eligibility threshold, evidence of a significant but quantitatively very small response along the intensive earnings margin (work effort or earnings manipulation through tax avoidance). There is also evidence of sharp bunching of durations of stay at the three year duration response. In the end, the migration elasticity is much larger than the conventional within country elasticity of earnings with respect to the net-of-tax rate (cf. Chapter 3). Hence, preferential tax schemes for highly paid workers could generate harmful tax competition across European countries and severely limit the ability of European government to use progressive taxation.

Summary in Danish (resumé)

Denne afhandling består af fire selvstændige kapitler, der alle omhandler emner inden for moderne arbejdsmarkedsøkonomi.

Kapitel 1: Not All Technologies Are Alike: Distinguishing the Effects of Information Technology and Communication Technology on Skill Demand

Eksisterende empiriske studier af effekten af teknologiske ændringer på virksomhedernes arbejdskraftefterspørgsel antager typisk, at alle typer af ny teknologi har den samme effekt på virksomhedernes efterspørgsel efter arbejdskraft.

I dette kapitel opstilles og testes en alternativ hypotese om, at informationsteknologi (IT) øger den relative efterspørgsel efter højtuddannet arbejdskraft, mens kommunikationsteknologi (KT) reducerer den relative efterspørgsel efter højtuddannet arbejdskraft. Baggrunden for den hypotese er ganske enkel. Gennem forbedret adgang til information, vil IT tendentielt øge kvalifikationskravene til virksomhedernes medarbejdere, idet de skal være i stand til at håndtere mere information og løse flere af de opgaver, de står over for, uden støtte fra andre. Ved at fremme kommunikationen, vil CT omvendt have tendens til at reducere de kvalifikationskrav, virksomhederne stiller til deres medarbejdere, idet medarbejderne har mulighed for at specialisere sig yderligere og være mere afhængige af andre, når de løser opgaver.

Hypotesen testes ved hjælp af et datasæt, hvor det er muligt at følge danske virksomhedernes brug af IT, KT og arbejdskraft over årene 2002-2008. Den empiriske analyse understøtter hypotesen om, at stigninger i IT øger den relative efterspørgsel efter højtuddannet arbejdskraft, mens stigninger i KT reducerer den relative efterspørgsel efter højtuddannet arbejdskraft. Dette resultat er særdeles robust. Desuden viser analysen, at efterspørgslen efter højtuddannet arbejdskraft er drevet af ændringer i både beskæftigelse og lønninger. Endeligt viser analysen, at størrelsesordenen af disse effekter ikke er ubetydelige: IT kan forklare ca. 24 procent af den ændrede efterspørgsel efter højtuddannet arbejdskraft, mens KT kan forklare ca. -10 procent. På denne baggrund konkluderes, at IT and KT har modsatrettede effekter på efterspørgslen efter højtuddannet arbejdskraft.

Dermed peger analysen også på, at det potentielt kan føre til fejlslutninger, hvis man ikke skelner mellem effekterne af IT og KT, når man vurderer effekterne af ny teknologi.

Kapitel 2: Do Human Arts Really Offer a Lower Return to Education? *(skrevet sammen med Carl-Johan Dalgaard og Anders Sørensen)*

I dette kapitel undersøges, om den lønforskel, der kan observeres for personer med humanistiske uddannelser og andre kandidatretninger, skyldes uddannelsestyperne i sig selv eller underliggende evner, der på samme tid påvirker valget af uddannelsestype og lønnen på arbejdsmarkedet. Simple OLS regressioner viser, at humanister i gennemsnit har en timeløn, der er omkring 20 procent lavere end kandidater med andre uddannelsesretninger.

Dog kan man have mistanke om, at de underliggende faktorer, der påvirker personers uddannelsesvalg, også påvirker vedkommende lønindkomst, uafhængigt af denne uddannelse. Hvis det er tilfældet, vil simple OLS regressioner for afkastet af uddannelse være misvisende, idet hele lønafkastet tilskrives kandidatretningen i sig selv, selvom den også skyldes andre bagvedliggende faktorer. For at løse dette potentielle selektionsproblem anvender vi instrumenter for valget af uddannelsestype. Vi dokumenterer, at den enkeltes uddannelsesvalg bliver påvirket af ældre elevers uddannelsesvalg på vedkommendes gymnasium. Når der tages højde for hvilket gymnasium, den enkelte gik på, påvirker ældre elevers uddannelsesvalg næppe yngre elevers løn efter de har opnået en kandidatgrad, og dermed udgør det et plausibelt instrument for valg af uddannelse.

En smule overraskende finder vi, at lønforskellen mellem humanister og andre kandidater bliver statistik insignifikant, når man instrumenterer for valget af uddannelse. Dette resultat tyder således på, at lønforskellen mellem personer med humanistiske uddannelser og andre kandidatretninger snarere skyldes selektionen indtil uddannelsestyper end selve uddannelserne i sig selv.

Kapitel 3: Estimating Taxable Income Responses Using Danish Tax Reforms *(skrevet sammen med Henrik Jacobsen Kleven)*

I dette kapitel estimeres elasticiteten af skattepligtige indkomster ved hjælp af registerdata for hele den danske befolkning i perioden 1984-2005. Den variation i skatterne, som

anvendes til at identificere effekterne af ændrede skatter, stammer fra en række danske skattereformer, der har skabt betydelig skattevariation både over tid og på tværs af individer og indkomstformer. Variationen i skatterne fra de danske skattereformer har ikke den samme stærke korrelation mellem skatteændringer og indkomstniveau som de skattereformer i USA og andre lande, der har ligget til grund for mange af de tidligere studier af elasticiteten af skattepligtige indkomster. Det giver os mulighed for at overvinde de identifikationsproblemer, der har plaget tidligere studier og som opstår som følge af ikke-skattedrevne ændringer i indkomstfordelingen og mean-reversion. I modsætning til tidligere studier, er vores resultater meget robuste overfor valget af kontrol for ikke-skattedrevne ændringer i indkomstfordelingen og mean-reversion. Det peger på, at vi er i stand til at kontrollere tilstrækkeligt for faktorer, der påvirker skattepligtige indkomster, men som ikke er direkte relateret til ændringer i skattesystemet. Kombinationen af de danske registerdata og de gennemførte skattereformer giver os således mulighed for at opnå en troværdig identifikation af de adfærdsmæssige effekter af beskatning.

Vores vigtigste resultater er følgende. For det første finder vi, at elasticiteten af arbejdsindkomst er ganske beskeden set over hele perioden. For lønmodtagere er den omkring 0,05, mens den for selvstændige er 0,10. For det andet er elasticiteten af kapitalindkomst to-tre gange større end elasticiteten af arbejdsindkomst. For det tredje viser vi, at adfærdsmæssige effekter (både for arbejds- og kapitalindkomst), der estimeres på baggrund af store skatteændringer, er væsentligt større end adfærdsmæssige responser, der estimeres på baggrund af små skatteændringer. Dette er konsistent med idéen om, at effekten af små skatteændringer reduceres på grund af optimeringsfriktioner (fx tilpasningsomkostninger), mens store skatteændringer i højere grad må forventes at overvinde sådanne friktioner og dermed ligge tættere på den faktiske, langsigtede elasticitet. Endeligt finder vi, at krydsprisindeffekterne mellem arbejds- og kapitalindkomst generelt set er små. Der er dog tendens til, at indkomstflytning mellem indkomstformer er mere udbredt blandt selvstændige end blandt lønmodtagere.

Kapitel 4: Taxation and International Migration of Top Earners: Evidence from the Foreigner Tax Scheme in Denmark (*skrevet sammen med Henrik Jacobsen Kleven, Camille Landais and Emmanuel Saez*)

Med udgangspunkt i bruttoskatteordningen for forskere og nøglemedarbejdere i Danmark (ofte betegnet *forskerordningen*), undersøges i dette kapitel effekten af indkomstbeskatning på migrationsbeslutningen blandt højtlønnede. Ordningen, som blev indført i 1991, giver udlændinge (og under særlige omstændigheder også hjemvendte danske statsborgere) med høje lønninger (over 103.000 Euro om året i 2009) mulighed for at lade sig beskatte med 25 pct. af bruttoindkomsten i en periode på op til 3 år i stedet for det almindelige progressive skattesystem, hvor den øverste marginalskat er på 59 pct. På baggrund af registerdata for hele befolkningen finder vi, at ordningen har fordoblet antallet af højtlønnede udlændinge i Danmark relativt til antallet af udlændinge med lønninger lige under lønkravet. Dette resulterer i en migrationselasticitet med hensyn til efterskattesatsen på over én. Vi finder også en ophobning af lønningen lige omkring ordningens lønkrav, hvilket er bevis for en signifikant, men kvantitativt set ganske lille respons langs den intensive margin (arbejdsindsats eller lønmanipulation via skatteunddragelse). Endeligt finder vi, at der sker en betydelig ophobning af varigheden af opholdet i Danmark lige omkring de 3 år. I sidste ende er migrationselasticiteten betydeligt højere end traditionelle indkomstelasticiteter, der ikke tager højde for migrationsbeslutningen (jf. Kapitel 3). Derfor er der risiko for, at ordninger, der begunstiger højtlønnede skattemæssigt, kan skabe skadelig skattekonkurrence på tværs af især europæiske lande og dermed begrænse de enkelte landes mulighed for at anvende progressiv beskatning.

Chapter 1

Not All Technologies Are Alike: Distinguishing the Effects of Information Technology and Communication Technology on Skill Demand

Esben Anton Schultz

Not All Technologies Are Alike: Distinguishing the Effects of Information Technology and Communication Technology on Skill Demand*

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Abstract

Existing empirical studies on the effect of information and communication technologies (ICTs) on skill demand assumes that ICT is one homogenous technology. However, this paper provides empirical evidence that ICT is a heterogeneous technology. Specifically, I propose and test the alternative hypothesis that information technologies (ITs) complement skills and communication technologies (CTs) substitute skills. Using a novel panel data set on Danish firms over the years 2002-2008, I offer support for this hypothesis. I document that IT leads to skill-upgrading, while CT leads to skill-downgrading. These effects are being driven by changes in both employment and wages. I also show that these effects are non-trivial: IT accounts for about 24 percent of the growth in skill demand over the period 2002-2008, while CT accounts for up to -14 percent of this growth. These findings demonstrate that IT and CT have heterogeneous effects on skill demand. More generally, these results suggest that relying on aggregate measures of ICT to study the effects of new technologies could be a serious error.

Keywords: skill demand; information technology; communication technology.

JEL Classification codes: C23; J23; O33.

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1 Introduction

The effects of information and communication technologies (ICT) on skill demand are important inputs in macroeconomic models and policy analysis. More than two decades of empirical studies document a strong positive relationship between ICT and the relative demand for high-skilled labor (e.g., Berman, Bound and Griliches 1994; Autor, Katz and Krueger 1998; Machin and Van Reenen 1998). The traditional interpretation of this evidence is that ICT is complementary with human capital and rapid falls in quality-adjusted ICT prices have therefore increased the relative demand for high-skilled labor. This has led to the hypothesis of technology-skill complementarity (see Goldin and Katz 1998 for a detailed discussion of this hypothesis).¹

Recently, a more nuanced view has been proposed by Autor, Levy and Murnane (2003), who argues that ICT substitutes for routine tasks that can be easily described by explicit rules but complements non-routine tasks that are not readily programmable. Because most non-routine job tasks are performed by either low-skilled labor (e.g., housekeepers and cab drivers) or high-skilled labor (e.g., consultants and physicians) and many routine job tasks are performed by medium-skilled labor (e.g., bank clerks), this hypothesis implies a "polarization" of the labor market with employment shifting away from the medium-skilled jobs and towards both the high-skilled and the low-skilled jobs. A large number of recent papers have provided evidence that is broadly consistent with this "ICT-induced polarization" hypothesis (e.g., Goos and Manning 2007, Spitz-Oener 2006, Goos, Manning and Salomons 2009 and 2010, and Michaels, Natraj and Van Reenen 2010).

However, despite being simple and intuitively attractive, both the technology-skill complementarity hypothesis and the ICT-induced polarization hypothesis rest on the ba-

¹Another often cited force behind these demand shifts is skill-biased technological change (SBTC). However, although SBTC and technology-skill complementarity are often confused in the literature, their underlying nature is substantially different. Simply put, SBTC is related to changes in unobservable factors and provides information on how technology affects the relative efficiency of inputs in the production process, while technology-skill complementarity is related to changes in observable factors and yields information on which inputs in the production process that can be easily substituted and complemented. Yet another technology-based explanation is the skill-biased organizational change hypothesis, suggesting that organizational changes provide the link between technology and skill demand (e.g. Caroli and Van Reenen 2001, Bresnahan, Brynjolfsson and Hitt 2002).

sic assumption that ICT is a homogenous technology. In this paper, I question whether this assumption is indeed reasonable. Specifically, I test the alternative hypothesis that information technology (IT) complements human skills and communication technology (CT) substitutes for human skills. The reasoning underlying this hypothesis is straightforward. On the one hand, by improving access to information, IT empowers workers to handle more of the problems they face without relying on others, thereby increasing the variety of tasks they have to perform in their jobs. On the other hand, CT makes it easier for workers to communicate with each other, which in turn generates a reduction in the variety of tasks workers have to deal with, as they can specialize further and rely more on others. Consequently, IT and CT have differential impacts not only on the variety of tasks but also on the associated responsibility and uncertainty workers face in their jobs. Because it is generally believed that a higher level of human capital of workers increases their ability to deal more effectively with an increased variety of job tasks, responsibility and uncertainty, this suggests that IT complements skills and CT substitutes for skills. Figure 1 provides a simple illustration of the hypothesis.

The idea that IT and CT are two distinct components of ICT has been proposed in recent work. Based on a similar reasoning as above, Bloom et al. (2010) argue that IT decentralizes firms' decision making while CT centralizes firms' decision making. Using a data set of firms in the U.S. and seven Continental European countries, they provide empirical evidence supporting their view. Moreover, Garicano and Rossi-Hansberg (2006 and 2010) show theoretically that improvements in IT leads to implications for wage inequality, firm organization and growth that are opposite to those from improvements in CT. However, although there seems to be a growing interest in the underlying characteristics of different components of ICT, there is no evidence on whether as well as how IT and CT separately affect skill demand. In this paper, I provide empirical evidence on this subject by testing the hypothesis that IT leads to skill upgrading and CT leads to skill downgrading.

I empirically test the hypothesis using a panel data set of Danish firms over the years 2002-2008 that merges an administrative matched employer-employee data set with ICT

expenditure surveys. These data combine three key advantages relative to the data that have been used in previous studies on the impact of ICT on skill demand. First, the data contain detailed information on skill characteristics and wages for each worker at a given firm. Second, the data contain disaggregated ICT expenditures on various subcomponents of ICT at the firm level, which allow me to distinguish between IT and CT. Finally, as the data cover five industries (manufacturing, construction, trade, transportation and service), the analysis is not limited to focusing on manufacturing as most previous studies.

As no natural experiment exists, I rely on differential changes in the quality-adjusted prices of IT and CT to identify the effects of IT and CT on skill demand. The idea behind this empirical strategy is that differential changes in the quality-adjusted prices of IT and CT create different incentives for firms to adopt IT and CT. These differential changes in IT and CT prices are far from negligible. To illustrate this, Figure 2 shows the evolution of IT and CT prices in the U.S. As the figure makes clear, the price of IT has decreased enormously over the past couple of decades, whereas the price of CT has been very stable.² Nevertheless, a potential problem with this empirical strategy is that omitted variables and measurement error may still explain some of the correlations I find, so my results should be considered as suggestive correlations rather than causal effects. However, employing an instrumental variable (IV) strategy using industry-level measures of IT and CT as instruments for firm-level IT and CT does not seem to alter the simple OLS results.

The empirical analysis provides evidence that confirms my hypothesis. Using a translog cost function framework, I find that increases in IT increase a firm's relative demand for skills, while increases in CT reduce a firm's relative demand for skills. I show that these effects are being driven by changes in both employment and wages. I also document that these effects are non-trivial: IT accounts for about 24 percent of the growth in skill demand, while CT accounts for about -14 percent of this growth.

Taken as a whole, the empirical analysis suggests that IT leads to skill-upgrading and

²Unfortunately, similar disaggregated data does not exist for Denmark. However, given the common availability of ICT it seems reasonable to expect a broadly similar evolution of IT and CT prices throughout the world.

CT leads to skill-downgrading and therefore shows that ICT indeed is a heterogeneous technology in relation to skill demand. More generally, this paper provides important new insights into our understanding of how new technologies translate into higher demand for skills.

Existing empirical studies on the skill content of technological change typically use aggregate measures of ICT, often simply computers per employee or the share of ICT expenditures in total investment or capital. The reason for this lack of attention on the different subcomponents of ICT is probably due to data constraints, as these data are rarely available, especially at the firm level. However, as I document in this paper, failing to distinguish between IT and CT could potentially be problematic as the two can be expected to have the opposite effects on skill demand. A few recent papers focus on the consequences of distinct types of ICT on productivity (Athey and Stern 2002, Jensen 2007, Garicano and Heaton 2010), but there have been no attempts to focus directly on skills. One of the main contributions of this paper is that I have access to data with which I can follow firms' use of IT, CT and skills over time.

The paper is organized as follows. The next section presents a simple skill demand framework and derive an estimating equation that forms the basis for the empirical analysis. Section 3 describes the Danish data and provide some institutional background. Section 4 presents the empirical results, while section 5 concludes.

2 Empirical Strategy

My goal is to measure the distinct impacts of changes in IT and CT on skill demand. To do so, I use a translog cost function framework with two labor inputs (low-skilled and high-skilled labor) and three capital inputs (IT, CT and non-ICT capital). This framework is very useful for testing the hypothesis that IT complements skills and CT substitutes skills because it allows different patterns of substitution and complementarity between low-skilled and high-skilled labor. From this basic model, I derive an estimating equation and discuss various identification issues.

2.1 Simple Model

Consider the short-run cost function C :

$$(1) \quad C = c(W^H, W^L, IT, CT, K, Y)$$

where C is the total cost, W^H is the wage of high-skilled labor, W^L is the wage of low-skilled labor, IT is IT capital, CT is CT capital, K is non-ICT capital and Y is value added. Following Brown and Christensen (1981), I assume that the only variable inputs are the two skill groups, whereas all types of capital are quasi-fixed. The translog cost function can now be defined as a second-order Taylor's series approximation in logarithm to an arbitrary cost function. Differentiating the translog cost function by the wage of each skill group, W^N , and using Shephard's Lemma, I obtain the following two wage-bill share equations:

$$(2) \quad S^H = \varphi_{HH} \ln W^H + \varphi_{LH} \ln W^L + \alpha_{ITH} \ln IT + \alpha_{CTH} \ln CT + \alpha_{KH} \ln K + \alpha_{YH} \ln Y$$

$$(3) \quad S^L = \varphi_{HL} \ln W^H + \varphi_{LL} \ln W^L + \alpha_{ITL} \ln IT + \alpha_{CTL} \ln CT + \alpha_{KL} \ln K + \alpha_{YL} \ln Y$$

where S^N is the wage-bill share of skill group $N = \{H, L\}$. By imposing the standard restrictions of symmetry and homogeneity of degree one in prices, one equation becomes redundant and the wage-bill share equation for high-skilled labor can be written as³:

$$(4) \quad S^H = \varphi_{HH} \ln(W^H/W^L) + \alpha_{ITH} \ln IT + \alpha_{CTH} \ln CT + \alpha_{KH} \ln K + \alpha_{YH} \ln Y$$

The model implies that the skill structure of a firm's workforce, given the level of output Y , depends on IT capital, CT capital, non-ICT capital and the relative wages between high-skilled labor and low-skilled labor. The hypothesis that IT complements for skills and CT substitutes for skills simply implies that $\alpha_{ITH} > 0$ and $\alpha_{CTH} < 0$.

³The estimates of the wage-bill share equation for low-skilled labor can be recovered from the symmetry restriction without estimating the equation.

2.2 Empirical Specification

For firm i at time t , the stochastic counterpart of the model is given by:

$$(5) \quad S_{it}^H = \alpha_i + \varphi_{HH} \ln(W^H/W^L)_{it} + \alpha_{ITH} \ln IT_{it} + \alpha_{CTH} \ln CT_{it} + \alpha_{KH} \ln K_{it} \\ + \alpha_{YH} \ln Y_{it} + \varepsilon_{Hit}$$

where α_i is the firm-specific fixed effects and ε is the error term.

There are a number of econometric problems with estimating equation (5). First, unobserved heterogeneity is likely to be correlated with both inputs and outputs (Mundlak 1961). For example, firms with better managers may find it easier to increase IT and reduce CT as well as rely less on low-skilled labor. To remove this "managerial ability" (Bloom and Van Reenen 2007), I estimate the wage-bill share equation for high-skilled labor in first-differenced form. Furthermore, I also include a set of firm characteristics (firm size, union density, and export intensities) as well as industry-year and region-year dummies. Moreover, I compare the wage-bill share equation with the employment share equation to check whether the results are being driven by changes in employment, wages or both.

Another issue is the treatment of wage variables. Since there is a direct relation between the wage-bill share of high-skilled labor and wages, it is very likely that wages are highly endogenous. Furthermore, wage variation across firms can be confused with unobservable differences in labor quality differences. In other words, there is assumed to be little exogenous variation in wages that is useful for the present analysis. Following most of the previous literature that uses a similar framework (e.g., Machin and Van Reenen 1998, and Caroli and Van Reenen 2001), I assume that labor markets are local in scope and replace the relative wage term with region-year dummies. Any remaining time-invariant firm-specific differences in relative wages are assumed to be captured in firm-specific fixed effects.

A third concern is the endogeneity of IT and CT. If IT and CT expenditures are chosen on the basis of economic incentives, they are unlikely to be independent of the factors

that influence the firm’s decision to employ workers in different skill groups. As a result, the unobservable factors that influence the skill structure of firm’s workforce are likely to be correlated with IT and CT, and the estimated coefficients may suffer from endogeneity biases. A simple way to try to deal with this problem is to use lagged values of IT and CT. However, if the capital stocks of IT and CT are persistent over time, it makes little difference for the results whether I use the lagged or contemporary values of IT and CT. Furthermore, because the use of lagged values significantly reduces the sample size, I estimate the baseline model using current values of IT and CT. As robustness checks, I try to deal with endogeneity using both one-year and two-year lagged values of IT and CT as well as employing an IV approach.

Finally, another important concern is measurement error in IT and CT, causing attenuation bias. As with endogeneity, I try to deal with measurement error using instruments for IT and CT.

The basic wage-bill equation of high-skilled labor can now be written as:

$$(6) \quad \begin{aligned} \Delta S_{it}^H &= \beta_1 \Delta \ln IT_{it} + \beta_2 \Delta \ln CT_{it} + \beta_3 \Delta \ln K_{it} + \beta_4 \Delta \ln Y_{it} \\ &\quad + \eta_1 x_{it} + \delta_1 IND_{jt} + \delta_2 REG_{kt} + u_{Hit} \end{aligned}$$

where Δ is the first-difference operator, i indexes the firm, t the time period, j the industry and k the region. x is a vector of firm characteristics, IND is industry-year specific dummies and REG is region-year specific dummies. In the baseline model, I estimate equation (6) using ordinary least squares (OLS) on the panel with standard errors clustered by firm and assume that all the correlated heterogeneity is captured by the firm-specific fixed effects and control variables.

3 Data and Institutional Background

The empirical analysis is based on Danish firm-level data. Denmark offers an ideal environment to study the impact of ICT on firm’s demand for skills because of the institutional

settings of the Danish labor market and its excellent micro data. The Danish labor market is characterized by what is commonly described as "flexicurity" - low employment protection, large active labor-market programs, and high levels of social insurance and transfers. This has led to turnover rates and average tenures in line with those of the Anglo-Saxon countries (Nicoletti et. al., 2000; OECD, 2002). However, there are important differences with respect to institutions and wage formation. The Danish labor market is heavily unionized, and the wage structure is relatively compressed, even for continental European standards (OECD, 2002). The Danish labor market thus shares some characteristics with other continental European labor markets and others with Anglo-Saxon labor markets. At the same time, Denmark has experienced large increases in ICT investments in recent decades, similar to those of the Anglo-Saxon countries (e.g., Severgnini, 2009). The results presented below may therefore also have implications for several other countries.

To test my hypothesis, I use a very rich panel of Danish firms that runs from 2002 to 2008. The data set combines matched employer-employee data - the so-called FIDA database - with ICT Expenditure Questionnaire Surveys, all administered and maintained by Statistics Denmark. For each firm, the data set contains complete information on workers' skill characteristics, value added, total capital stock and various subcomponents of total ICT expenditures. In addition, the data set contains a set of firm characteristics, such as firm size, industry and union density. I describe the data sources and the main variables in greater detail below.

3.1 The FIDA Database

The FIDA database is a matched employer-employee panel data set covering the entire population of firms and individuals in Denmark over the years 1995-2008. Each individual and each firm is associated with a unique identification code, and most importantly, all employed individuals are linked with a firm through the firm identification code at the end of the year. The individual level variables are extracted from the integrated database of labor market research (IDA database) and the Danish income registry in Statistics

Denmark. For more details on the IDA database, see Abowd and Kramarz (1999). For each individual, I have complete data on educational attainment and wages along with detailed information on other socioeconomic characteristics on an annual basis. The FIDA database also provides information on value added and the capital stock measured by the value of fixed assets on a firm's balance sheet.

3.1.1 Measuring Skills

To measure the skill-level of the workforce, I divide workers into two skill groups based on their formal level of education: low-skilled labor and high-skilled labor. The Danish educational system consists of a vocational track and an academic track. Vocational education is a mix of schooling and training in firms with a typical duration of three years and also includes workers with a short academic education. Academic education includes workers with medium academic education, corresponding to a bachelor's-level degree, and workers with high academic education, corresponding to a master's-level degree or more. For the present purpose, I define high-skilled labor as those workers with medium or high academic education, whereas low-skilled labor is defined as those with vocational education or no education. This definition of skill groups is very similar to the college/non-college grouping of skill groups used in many previous studies (e.g., Krusell et al. 2000).

3.2 The ICT Expenditure Questionnaire Survey

The ICT Expenditure Questionnaire Survey has been collected annually by Statistics Denmark since 2003 and covers a proportional stratified random sample of about 3,000 private Danish firms with at least 10 employees in each year. The sample is stratified by employment size (number of employees) and industry, and the average response rate is 95 percent. I have access to the survey from 2003 to 2008. These data are originally collected to form the ICT investment series in the Danish National Accounts. For this reason, Statistics Denmark performs extensive quality checks on these data, which ensures

high levels of accuracy. Moreover, although the survey is not specifically designed as a longitudinal survey, its broad coverage makes it possible to identify many firms that have participated in the survey more than once over this six-year period.

The survey is targeted at managers responsible for a firm's ICT investments and simply asks how their firm's total ICT expenditures are distributed across various subcomponents of ICT, measured in prices. These subcomponents include: hardware (computers, screens, keyboards, printers, and scanners), communication equipment (network equipment, cabling, phones, faxes, antennas), audio-visual equipment (televisions, DVDs, projectors, cameras), other IT-equipment (measuring instruments, apparatus for research purposes), standard software (software-packages, operative systems, word processing, spreadsheets), custom software (customized software, ordered and bought by the firm), IT-service (consultancy, webhosting, maintenance contracts) and IT-training (external IT-training). To ensure that the respondents fill out the questionnaire correctly, it contains detailed instructions, including a description of what is meant by each subcomponent of ICT. Furthermore, if the respondent does not know the exact value of the firm's investment in a specific ICT component, they are asked to report their best estimate. Although the reported expenditures are thus not always completely accurate, there is no reason to believe a priori that these *reporting errors* should lead to any systematic bias both within and between different ICT components.

3.2.1 Measuring IT and CT

My hypothesis makes a sharp distinction between IT and CT. I argue that ITs are technologies that improve access to information, while CTs are technologies that ease communication. Ideally, I should therefore distinguish between technologies that only either improves information access or eases communication. In reality, however, this is not a straightforward task, as some technological innovations may both improve information access and ease communication. Hence, to make the distinction as simple and transparent as possible, I therefore chose to measure CT as simply communication equipment, while IT consists of all the other subcomponents (hardware, audio-visual equipment, other

IT-equipment, standard software, custom software, IT-service and IT-training).

Admittedly, some of these subcomponents can be expected to both improve access to information and ease communication. In particular, it is obvious that hardware serves both functions. However, since the price of hardware is mainly determined by its capacity to store and process information, it seems *prima facie* reasonable to expect that the net effect of hardware on skill demand is positive. As a robustness check, I investigate whether this is actually the case.

Although they are not perfect, these measures of IT and CT seem to capture the main idea of my hypothesis. Moreover, since the hypothesis implies that the coefficient of IT is positive and the coefficient of CT is negative, both coefficients would be biased toward zero if the applied measures of IT and CT consist of subcomponents that both improve information access and ease communication. As a result, it would make it harder to reject the hypothesis that IT leads to skill upgrading and CT leads to skill downgrading.

To measure the change in the capital stocks of IT and CT, I use IT and CT expenditures as a share of value added, $\frac{IT \exp_{it}}{Y_{it}}$ and $\frac{CT \exp_{it}}{Y_{it}}$.⁴ As shown by Griliches (1973) and Terleckyj (1980), this simple approach provides a good approximation of the actual change in the stock of new technologies between period $t - 1$ and t .⁵ Figure 3 depicts the evolution of these two measures during the period I consider. It is seen that IT expenditures as a share of value added has gradually increased over this period, while the CT expenditures as a share of value added has been fairly stable. This pattern is consistent with the idea that the fall in IT-prices relative to CT-prices creates a stronger incentive to invest in IT relative to CT, as suggested above.

⁴Machin and Van Reenen (1998) use a similar approach to measure change in the stock of R&D capital.

⁵Alternatively, I could have constructed IT and CT capital stocks from IT and CT expenditures using the perpetual inventory method. However, due to, among other things, the shortness of my panel, this method would most likely generate very imprecise estimates of the IT and CT capital stocks.

3.3 Sampling and Descriptive Statistics

I construct the analysis dataset by matching the FIDA database over the years 2002-2008 with the ICT Expenditure Surveys from 2003 to 2008 through the unique firm identification code. Starting with a sample of 18,051 observations and 9,961 firms, I exclude firms that could not be matched through the unique firm identification code, those who do not employ workers in both skill group, and those who do not have observations in at least two consecutive years in the FIDA database. While the second restriction is made to avoid a corner solution, the latter exclusion restriction is made to measure all variables in changes, as IT and CT are already measured in changes. These exclusions leave us with a sample that consists of an unbalanced panel of 12,523 observations and 6,434 firms that covers five industries (manufacturing, construction, trade, transportation and service). Although the sample only represents about 10 percent of all firms with more than 10 employees in these five industries each year, it represents about half of both total employment and total turnover. The sample is therefore not completely representative of the population of firms in these five industries, as large firms are highly oversampled. However, because larger firms tend to invest more in ICT per employee (e.g., Astebro 2002; Hubbard 2000)⁶, this sample may be particularly interesting to study.

Table 1 presents summary statistics for the main estimation sample. The mean number of employees for a firm in the sample is 230, and four out of five of these employees are members of a union. Most interesting for the purpose of this paper is that both the wage-bill and the employment share of high-skilled labor have increased, implying that skill-upgrading has taken place during this period. Moreover, IT represents about 5 percent of value added, while CT represents less than 1 percent of value added.

⁶The most common reasons for this relationship are economics of scale, slack resources, access to outside resources, and ability to bear adoption risks (Forman and Goldfarb 2006).

4 Empirical Results

In this section, I present the results of my estimations. Section 4.1 contains the main results. Section 4.2 considers a number of robustness checks. Section 4.3 presents some results on the magnitudes of the effects.

4.1 Main Results

The model is specified in first-differenced form, which implies that the parameters are identified using only variation in the explanatory variables within firms over time. Hence, the hypothesis is not that firms with the highest level of IT (CT) has the highest (lowest) skill-level of their workforce but rather that within a given firm an increase in IT (CT) will cause a change towards an increase (decrease) in the skill-level of their workforce.

Table 2 reports the results of the baseline model. In column 1, I estimate the model including only industry-year and region-year dummies. The coefficient on IT is positive and statistically significant although only at the 10 percent level, while the coefficient on CT is negative and significant at the 5 percent level. These results are consistent with my hypothesis that IT leads to skill-upgrading and CT leads to skill-downgrading. Turning to other variables, it is seen that the value added has a negative effect on the relative demand for high-skilled labor. The coefficient of non-ICT is negative but insignificant, suggesting that there is no evidence of non-ICT capital-skill complementarity. Many studies have found evidence of capital-skill complementarity using aggregate capital (e.g., Griliches 1969, Krusell 2000), but those studies that distinguish between ICT and non-ICT capital typically find that the effect of non-ICT capital is insignificant (e.g., Michaels, Natraj and Van Reenen 2010).

Column 2 replicates column 1 with a full set of covariates in addition to the industry-year and region-year dummies. These include firm size (number of employees), union density (share of workers with union membership), and export intensity (export as a share of value added). Again, the coefficient on IT is positive, and the coefficient on CT is negative. Both are still significant. Moreover, the estimates of the coefficients of

interest are virtually unaffected by the inclusion of this set of firm characteristics. Because controlling for observed heterogeneity has little impact on the estimates, one can be more confident that unobserved heterogeneity is unlikely to be driving the results.

The specification in equation (6) does not tell us whether the results obtained in columns 1 and 2 of Table 2 are driven by changes in quantities, factor prices or both. To try to distinguish between these two channels, I replace the wage-bill share of high-skilled labor with the employment share of high-skilled labor in equation (6). These results are reported in columns 3 and 4 of Table 2. Changes in IT and CT again have the same opposite effects on the relative demand for high-skilled labor as in column 1 and 2. In addition, the estimates in column 4 confirm that these results are robust to the inclusion of additional covariates. However, the coefficients on IT and CT are smaller in magnitude than those obtained using the wage-bill share, and both are insignificant. Although not conclusive because the distinct impact of wages has not been analyzed, this seems to suggest that changes in skill demand are being driving by both changes in wages and employment.

4.2 Robustness and Extensions

The results above suggest that IT leads to skill-upgrading and CT leads to skill-downgrading. There are many issues and extensions that follows from these results. In this section, I investigate some of them in further detail.

4.2.1 Alternative Specification and Samples

First, I replicate the baseline specification in column 2 of Table 2, but restrict the sample to those firms who have participated in the survey more than once. The objective of this specification is to assess whether the results I obtained for the full estimation sample are not simply being driving by those firms that only have participated in the survey once. The results of this exercise are reported in column 1 of Table 3. Again, the coefficient of IT is positive, and the coefficient of CT is negative. However, the point estimates are

larger in magnitude and even more significant than those obtained in column 2 of Table 2, although they are slightly less precisely estimated due to reduced sample size. By further restricting the sample to those firms that have participated in the survey in at least two consecutive years, column 2 of Table 3 reveals that the coefficients of IT and CT are similar in magnitude to those obtained column 1 of Table 3.

Second, IT and CT are likely to be endogenous. As an initial attempt to try to deal with this problem, I estimate the model using lagged values of IT and CT. The usual argument for using lagged values is that although current values of IT and CT might be endogenous to skill demand, past values might not suffer from this problem to the same degree. In column 3, I use one-year lagged values of IT and CT, and in column 4, I use two-year lagged values of IT and CT. The coefficients of IT and CT from both of these specifications take their expected signs and are significant. These findings confirm my hypothesis. However, it is unlikely that using one-year and two-year lagged values of IT and CT solves the endogeneity problem, especially if IT and CT capital stocks are persistent over time. Below, I therefore try to deal with endogeneity employing an IV approach.

Third, a concern may be that the results in Table 2 are solely driven by the particular data I use in this paper. To shed some light on this issue, I test whether I obtain results that are similar to the existing literature using only aggregate ICT. As can be seen from column 5 of Table 3, the estimated coefficient of ICT is positive and significant at the 5 percent level, implying that aggregate ICT increases the relative demand for skills. These results are in line with previous studies and suggest that the results in this paper do not reflect only a particular feature of the applied data or Danish firms. It also stresses the importance of distinguishing between IT and CT when testing the technology-skill complementarity hypothesis.

Fourth, there is the treatment of hardware. As argued above, although hardware both improves information access and eases communication, the net effect of hardware on the relative demand for skills is likely to be positive because the price of hardware is mainly determined by its capacity to store and process information. However, this may be

contentious. To check whether this is the case, I reestimate the model where ICT capital is divided into hardware and non-hardware ICT. Column 6 shows that the coefficient of hardware is positive and significant, although it is less precisely estimated than the effect of aggregate IT obtained in Table 2. It thus seems that the overall impact of hardware on skill demand is positive. Moreover, the effect of non-hardware ICT is insignificant.

4.2.2 Endogeneity and Measurement Error

As mentioned already, an obvious criticism of the results presented above is that there may be unobservables in equation (6) that are correlated with both the dependent variable and the capital stocks of IT and CT, which will bias the estimates. However, it is not obvious a priori that the results presented above are driven by endogeneity biases because although IT and CT are strongly positively correlated with each other in the data⁷, their predicted effects on the same dependent variable take opposite signs. For endogeneity to generate these results, it would require that the hypothetical unobservable variable that is positively correlated with the dependent variable has a positive covariance with IT and a negative covariance with CT. This is of course possible, but it is unclear what would generate this bias. Another concern is that the right-hand-side variables of equation (6), especially IT and CT, are plagued with measurement error, causing attenuation bias in the estimates.

To deal with these concerns, I invoke an IV strategy using industry-level measures of IT and CT as instruments for firm-level IT and CT. For each firm, these instruments are calculated as the mean value of (IT exp./value added) and (CT exp./value added) in the same industry as the firm, excluding the firm's own values of (IT exp./value added) and (CT exp./value added). These calculations are based on all the firms in the surveys. The idea behind this set of instruments is that differential falls in the quality-adjusted prices of IT and CT have different impacts on firms' adoption of IT and CT because the degree to which firms are reliant on IT and CT in their production process varies considerably.

⁷In the main estimation sample, the pair wise (Pearson) correlation between (IT exp./Y) and (CT exp./Y) is 0.79 and significant at the 1 percent level.

These instruments proxy this reliance.

The results of the IV estimations are reported in Table 4. In all specifications, the signs of the instruments in the first stage are as expected (not reported). Furthermore, the instruments are "strong" because they pass the threshold value of the Cragg-Donald statistics of 7.03, as suggested by Stock and Yogo (2005).

In column 1, I estimate the model for the full sample. It is seen that the two-stage least squares (2SLS) estimates of IT and CT have the expected sign and both are significant at the 1 percent level. Moreover, the point estimates are substantially larger in magnitude than their OLS counterparts reported in column 2 of Table 2. The standard errors are, however, also considerably larger than those obtained using OLS, which is often the price to pay when using instruments.

Column 2 of Table 4 replicates column 1 using only the sample of firms that have participated in the survey more than once. Again, it is seen that the 2SLS estimates are much larger than their OLS counterparts, although the point estimate of IT is smaller than in the previous 2SLS regression.

In column 3, I restrict the sample to those firms that have participated in the survey in at least two consecutive years. The 2SLS estimates are still much larger than their OLS counterparts. Finally, column 4 presents the 2SLS results where I use the instruments lagged one-year. It is seen that the results are similar to those obtained using the contemporary instruments.⁸ Taken together, these findings confirm the hypothesis that ITs increase the relative demand for skills, while CTs reduce the relative demand for high-skilled labor. Furthermore, these results are consistent with the presence of measurement error and suggest that I probably underestimate the importance of IT and CT by using only OLS.

⁸I have also estimated the model using GMM and LIML. These estimators produce similar results.

4.3 Magnitudes

Having established that both IT and CT have statistically significant effects on skill demand that are consistent with my hypothesis, I now evaluate the economic importance of these effects. To do so, I calculate the following formula:

$$(7) \quad Contribution_V = [\hat{\beta}_{Vit} * mean(\Delta V_{it}/Y_{it})]/mean(\Delta \Delta S_{Hit})$$

where V denotes IT and CT expenditures, respectively. Taking the difference of the difference means that equation (7) can only be calculated for those firms that have participated in the survey in at least two consecutive years.

Table 5 reports these results. Using OLS estimates column 1 shows that IT accounts for 4.2 percent and CT accounts for -1.0 percent of the increase in the wage-bill share of high-skilled labor. Therefore, IT and CT together explain only 3.2 percent of the variation in the wage-bill share of high-skilled labor during the period 2002-2008. As argued above, however, OLS estimates are likely to underestimate the importance of IT and CT because of measurement error in IT and CT. Turning attention to the 2SLS results in column 2, we observe a dramatic change. IT now accounts for 24.3 percent of the growth in the skill demand, while CT accounts for -13.6 of this growth. Thus, IT and CT now explain more than 10 percent of the observed variation in the skill demand. This marked difference in the estimated contributions simply reflects that the IV estimates of IT and CT are considerably larger than their OLS counterparts.

These simple calculations should only be considered as suggestive. However, due to measurement error and the fact that value added growth is likely to be strongly affected by IT and CT growth (e.g., Jorgenson, Ho and Stiroh 2008), these calculations will probably underestimate the importance of IT and CT. It thus seems that both IT and CT are important in explaining a significant proportion of the increased demand for high-skilled labor.

5 Conclusion

Existing empirical studies on the skill content of technological changes assumes that all types of ICT have the same effect on skill demand. In this paper, I propose the alternative hypothesis that IT complements skills, while CT substitutes for skills. Through improved access to information, IT will tend to increase the skill requirements of workers, as they have to handle more information and solve more of the problems they face without relying on others. Conversely, by facilitating communication, CT will tend to reduce the skill requirements of workers, as workers can then specialize further and rely more on others when solving problems.

Using a novel panel of Danish firms, I confirm this hypothesis. I document that increases in IT increase the relative demand for high-skilled labor, while increases in CT reduce the relative demand for high-skilled labor. I also show that the demand for skills is driven by changes in both employment and wages. Moreover, I show that these effects are non-trivial: IT accounts for about 24 percent of the change in skill demand, while CT accounts for about -14 percent. I thus conclude that IT and CT are indeed heterogeneous technologies in relation to skill demand. Moreover, the analysis suggests that failing to account for the differential impacts of IT and CT can potentially be a serious error.

Although I view this analysis as useful first step given the lack of evidence of this issue, there are many extensions in this line of research that could be pursued. First, additional empirical work analyzing natural experiments and/or using longer panels is needed to more carefully identify the causal effects of IT and CT on skill demand. Second, although I provide some evidence that suggests a more nuanced version of the ICT-induced polarization hypothesis, it would be a useful extension to consider occupational data to more precisely identify the relationship between that hypothesis and my hypothesis. Finally, an important area for future research is to study the distinct effects of different types of ICT on not only skill demand but also productivity, firm organization and wage inequality. Studies by Garicano and Rossi-Hansberg (2006 and 2010) and Bloom et al. (2010) have already made contributions in this area.

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Tables

TABLE 1
Summary Statistics of Main Estimation Sample

	Mean	Std. dev.
$\Delta(\text{high-skilled wage-bill share})$	0.005	0.048
$\Delta(\text{high-skilled employment share})$	0.004	0.044
$\Delta \ln(Y)$	0.087	0.413
$\Delta \ln(\text{Non-ICT capital})$	0.013	0.690
IT exp./Y	0.047	0.173
CT exp./Y	0.005	0.110
Export/Y	0.966	8.438
Union density	0.779	0.168
Number of employees/firm size	229.631	793.214
Manufacturing	0.325	
Construction	0.050	
Trade	0.268	
Transportation	0.076	
Service	0.281	
Number of observations	12,523	
Number of firms	6,434	

TABLE 2
Effects of IT and CT on Skill Demand: Ordinary Least Squares

Dependent variable	Changes in High-Skilled Labor:			
	Wage-Bill Share		Employment Share	
	(1)	(2)	(3)	(4)
$\Delta \ln(Y)$	-0.008*** (0.003)	-0.008*** (0.003)	-0.008*** (0.002)	-0.008*** (0.003)
$\Delta \ln(\text{Non-ICT capital})$	-0.000 (0.001)	-0.000 (0.001)	-0.001 (0.001)	-0.001 (0.001)
IT exp./Y	0.011* (0.006)	0.011* (0.006)	0.007 (0.006)	0.007 (0.006)
CT exp./Y	-0.015** (0.007)	-0.014** (0.007)	-0.002 (0.008)	-0.002 (0.008)
Firm size		0.000 (0.000)		0.000 (0.000)
Union density		-0.008*** (0.003)		-0.006* (0.003)
Export /Y		0.000 (0.000)		0.000 (0.000)
Industry-year dummies	x	x	x	x
Region-year dummies	x	x	x	x
Observations	12,523	12,523	12,523	12,523

Notes: Standard errors within paratheses. * = significant at the 10% level, ** = significant at the 5% level, *** = significant at the 1% level. Estimation is by OLS, where the error terms are allowed to be correlated within firms.

TABLE 3
Robustness Checks and Selection Tests

Sample of firms/specification	Participated in survey in at least two consecutive years					Distinct effect of hardware
	(1)	(2)	(3)	(4)	(5)	
Dependent variable	Changes in Wage-Bill Share of High-Skilled Labor					
$\Delta \ln(Y)$	-0.010*** (0.003)	-0.013*** (0.004)	-0.013*** (0.004)	-0.011*** (0.003)	-0.008*** (0.003)	-0.008*** (0.003)
$\Delta \ln(\text{Non-ICT capital})$	0.000 (0.001)	0.002 (0.001)	0.002 (0.001)	0.002 (0.001)	-0.000 (0.001)	-0.000 (0.001)
IT exp./Y	0.019** (0.008)	0.020*** (0.007)	0.017** (0.008)	0.013* (0.007)		
CT exp./Y	-0.037*** (0.012)	-0.027** (0.013)	-0.020* (0.011)	-0.076*** (0.010)		
ICT exp./Y					0.004** (0.002)	
Non-hardware ICT exp./Y						-0.001 (0.002)
Hardware exp./Y						0.025* (0.015)
Observations	8,847	4,891	4,891	3,427	12,523	12,523

Notes: Standard errors within parentheses. * = significant at the 10% level, ** = significant at the 5% level, *** = significant at the 1% level. All specifications include controls for firm size, union density and export share as well as industry-year and region-year dummies. Estimation is by OLS, where the error terms are allowed to be correlated within firms.

TABLE 4
Effects of IT and CT on Skill Demand: Instrumental Variable Estimates

Sample of firms	All	Participated in survey more than once	Participated in survey in at least two consecutive years
Instruments	Industry IT and CT	Industry IT and CT	One-year lagged industry IT and CT
Dependent variable	(1)	(2)	(3)
Changes in the Wage-Bill Share of High-Skilled Labor:			
$\Delta \ln(Y)$	0.002 (0.005)	-0.009** (0.004)	-0.007* (0.006)
$\Delta \ln(\text{Non-ICT capital})$	-0.001 (0.001)	-0.000 (0.001)	0.001 (0.001)
IT exp./Y	0.287*** (0.071)	0.104** (0.042)	0.117** (0.049)
CT exp./Y	-0.421*** (0.138)	-0.527* (0.312)	-0.365* (0.190)
Cragg-Donald Statistics	15.48	12.86	13.45
Observations	12,523	8,847	4,891

Notes: Standard errors within parentheses. * = significant at the 10% level, ** = significant at the 5% level, *** = significant at the 1% level. All specifications include controls for firm size, union density and export share as well as industry-year and region-year dummies. Estimation is by 2SLS, where the error terms are allowed to be correlated within firms.

TABLE 5
Contributions of IT and CT to Changes in Skill Demand

	(1)	(2)
Panel A: Estimates of the Translog Cost Function		
Dependent variable	Changes in Wage-Bill Share of High-Skilled Labor	
	OLS	2SLS
IT/Y	0.020	0.117
CT/Y	-0.027	-0.365
Tables and columns used	Table 3, column 2	Table 4, column 3
Panel B: Difference of the Change		
$\Delta(\text{Changes in wage-bill share of high-skilled labor})$	0.0005	0.0005
$\Delta(\text{IT exp./Y})$	0.0011	0.0011
$\Delta(\text{CT exp./Y})$	0.0002	0.0002
Panel C: The Contribution of IT and CT		
Mean contribution of IT	4.2%	24.3%
Mean contribution of CT	-1.0%	-13.6%
Total contribution	3.2%	10.8%

Figures

FIGURE 1
A Sketch of the Hypothesis

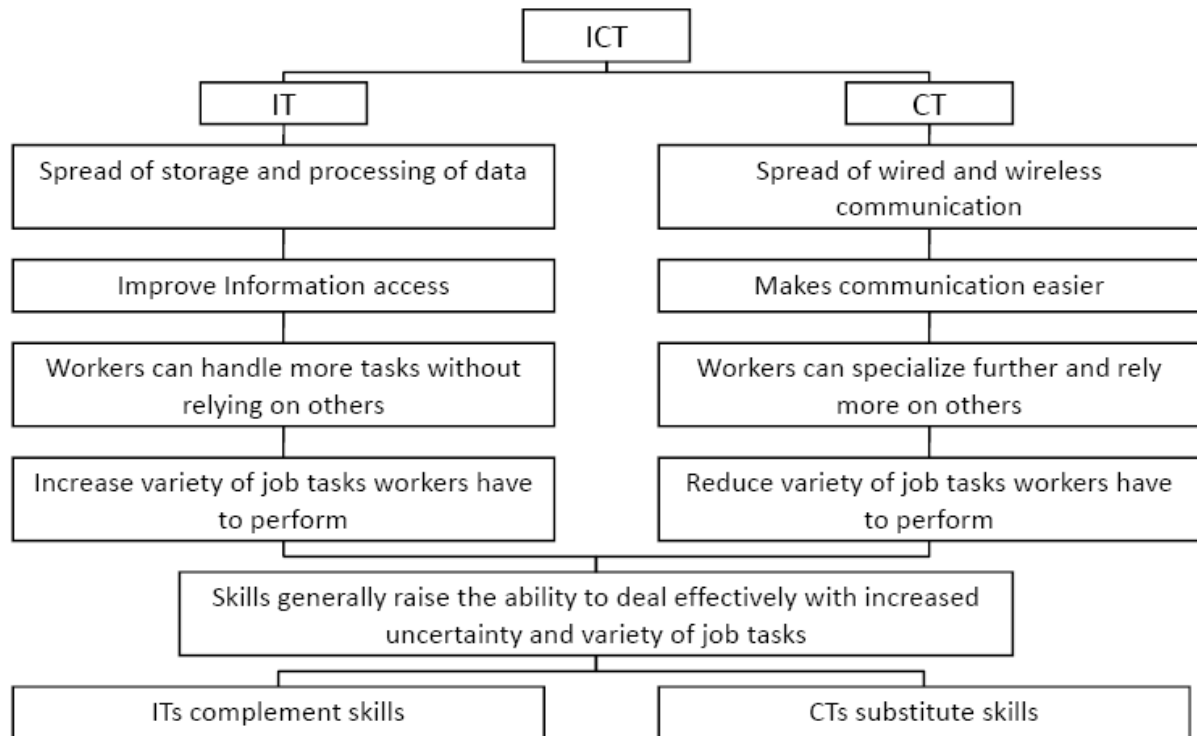
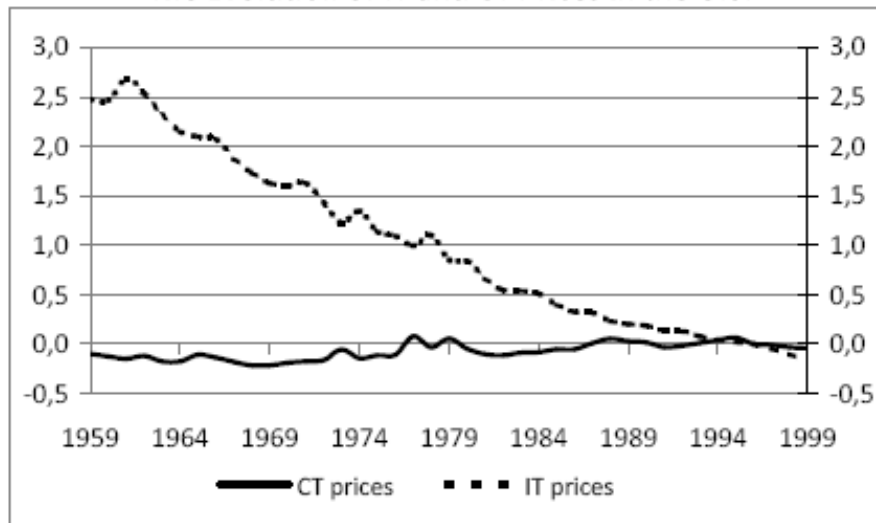
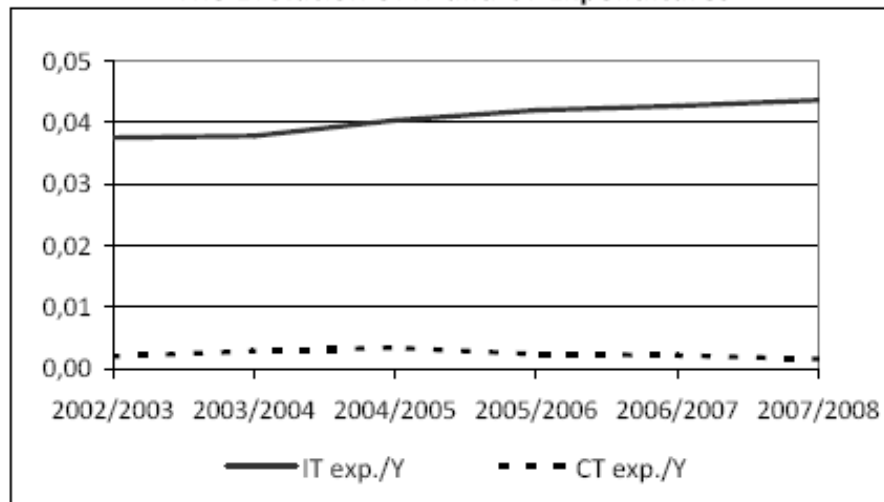


FIGURE 2
The Evolution of IT and CT Prices in the U.S.



Notes: Own calculations based on Table 4 in Jorgenson (2001). IT consists of computers and software, while CT consists of communications. Values in current U.S. dollars are used as weights. Prices are in logs, normalized to zero in 1996.

FIGURE 3
The Evolution of IT and CT Expenditures



Notes: Calculations are based on the main estimation sample.

Chapter 2

Do Human Arts Really Offer a Lower Return to Education?

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Do Human Arts Really Offer a Lower Return to Education?*

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Abstract

Is the wage gap between majors in human arts and other fields caused by their education *per se*? If the educational choice is endogenous, the wage gap may instead be caused by selection. We document that individuals' educational choice is correlated with that of older students, suggestive of peer effects. Conditional on high school fixed effects, this link is unlikely to affect post-university wages and thereby constitutes a plausible instrument for the educational choice. Our 2SLS estimates reveal that the gap in returns to education is negligible, implying that the wage gap is mainly attributable to selection.

Keywords: Returns to education, human arts, instrumental variables

JEL Classification codes: I21, J24

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1 Introduction

The notion that education is a key determinant of individual productivity has a long and distinguished history in economics, going back (at least) to the work of Mincer (1958), Houthakker (1959) and Miller (1960). At the conceptual level one may distinguish between three dimensions of a formal education which hold the potential to affect individual productivity: The quantity of education, the quality of education, and the subject matter studied.

While the quantity of education can be measured by years of schooling, the quality of education is harder to account for. Still, one may attempt to gauge the impact from quality by adding reasonable proxies to otherwise standard wage regressions, such as test score results. Alternatively, one may try to infer the impact from quality by including characteristics of the school attended in earnings regressions (e.g. pupil/teacher ratios and school size). As is well known, standard theories would predict a positive impact from both of these dimensions of education on individual productivity (Becker, 1967), as well as on macroeconomic outcomes (e.g. Lucas, 1988). This proposition has been tested (and debated) intensely over the years.¹

The third dimension of human capital accumulation, which has received considerably less attention by academic researchers, is what we focus on in the present study. The issue is whether the particular field of study, or the contents of the curriculum, has a separate impact on individual productivity. Existing studies, surveyed below, suggests this is the case. A typical finding is that the labor market pay-off from pursuing an education within the humanities is substantially smaller than that associated with most other types of education. For example, ordinary least squares (OLS) estimates for Denmark, reported below, suggests that the wage rate earned by individuals with a tertiary education within the humanities is 23% lower than that associated with other tertiary degrees.²

¹See Card (1999, 2001) for a review of the literature which attempts to estimate the causal impact from an additional year of schooling on individual wages; Card and Krueger (1996) review the literature on the impact from school quality on labor market outcomes at the level of the individual. Bills and Klenow (2000) provide an analysis of the education/growth nexus at the aggregate level; Hendricks (2002) examines the contribution from quality differences in human capital in accounting for cross-country wage differences.

²We refer to the groups under consideration as having obtained a “tertiary” education. Note that all individuals

These findings could suggest that some types of education provide the individual with more productive human capital than others. At the same time, large wage premia across different fields of study are somewhat puzzling. If wage differentials (of a considerable magnitude) appear one would *a priori* expect changes in the distribution of students across fields of study; a process that would continue (in theory) until wages are equalized.

An alternative explanation for the above mentioned findings is that existing OLS estimates are not identifying the impact of different types of education on wages. Instead, the results may be attributed to a lack of control for differences in relative cognitive abilities, or, “comparative advantages” in intellectual pursuits. It seems plausible that comparative intellectual advantages matter when the individual chooses which *type* of education to pursue. That is, a relatively mathematically skilled student may be more partial to pursue an education where mathematics is used intensively, compared to a gifted student with comparative advantages in verbal abilities. Moreover, some types of ability do seem to yield a higher labor market pay-off than others. For example, Dougherty (2003) finds that numeracy has a strong positive impact on individual wages, whereas literacy has a much smaller (and often insignificant) impact.³ Accordingly, if relative cognitive abilities determine the type of education, the individual pursues *and* affects the final wage, existing return estimates to the *type* of education may be biased.

The Danish educational system is well suited for studying the returns to different types of education. The reason is that university degrees in Denmark are highly specialized. For example, if one chooses to study economics then this is the subject matter pursued throughout the entire time at the university; both during the undergraduate and the graduate level. Intellectual excursions into other fields only occur to a very modest extent, in contrast to what may be the case under e.g. a US-type system. Consequently, examining the labor market performance of Danes holding

in our sample below have attained a master’s degree. Hence, the number of years of schooling for all individuals in our sample is rather homogenous.

³See also Bishop (1992) and Joensen and Nielsen (2009) who find that greater skills in mathematics goes along with higher individual level wages. Interestingly, similar results are obtained in the aggregate data. Hanushek and Woessmann (2009) document that the link between average test scores in mathematics and science is more strongly related to *aggregate* growth than test scores in reading; when all three types of test scores are included in the regression the latter turns insignificant.

different types of tertiary education is likely to convey information about the extent of human capital production within different fields of study. In addition, Danish universities are publicly funded which reduces the scope for marked quality differences.

Accordingly, the present paper contributes to the literature by attempting to elicit information about the causal effect of *the field of study* on individual productivity, as it manifests itself in individual wages. The data set underlying the empirical analysis covers the part of the Danish population which completed high school during the period 1981-1990.⁴ Narrowing the focus to the group of individuals which subsequently proceeded to a tertiary education, and ended up in wage-employment, we examine whether wage rates differ systematically across previous field of study. Specifically, we examine the relative labor market performance of individuals who chose to study within the broad fields of human arts and other types of tertiary educations.⁵ Conditional on standard determinants of wages an OLS regression reveals that individuals who pursued an education within the human arts fared much worse, as noted above, than individuals with other majors.

Still, OLS estimates are unlikely to capture the causal effect of the type of education on individual productivity, unless relative cognitive abilities are controlled for. Accordingly, we subsequently try to control for comparative intellectual advantages by invoking individual-level information about academic specialization in the Danish high school system. In addition, we are able to utilize information about the high school attended and high school GPA, as well as data on parental education. Upon including such controls in the wage equation, the wage gap between human arts majors and other majors narrows. Still, a significant difference persists; the wage difference between human arts majors and others is now 17% (reduced from 23%).

Ultimately it is hard to rule out that other – unobserved – factors could simultaneously impact

⁴When we refer to the Danish high school in this paper, we mean the ordinary high school ("gymnasium"). The Danish high school takes three years to complete.

⁵We have also attempted to examine a finer division of studies. Unfortunately, we have not been able to disentangle the returns to education in this more disaggregated setting; our instrument turns out to be weak in this setting. A possible interpretation is that we need a description of relative abilities in more dimensions than the two dimensional "verbal" versus "mathematical" ability division that we apply here.

on the choice of education type as well as productivity. As a result, we try to make additional headway by employing an instrumental variables (IV) approach to the issue at hand.

To identify the impact of the field of study on wages, we begin by studying the educational choice itself. That is, the choice of which type of tertiary education to pursue. Specifically, we model the choice of field of study as a function of (relative) academic abilities, and variables thought to capture the observed academic tastes of the individuals' high school peer group. While the former turns out to be linked to final wages, the latter determinants should not affect the productivity of the individual, once we carefully control for high school fixed effects (perhaps reflecting variation in teacher quality etc.), the curriculum studied by the individual in high school and the academic achievements of the individual when graduating from high school. As a consequence, peer group characteristics may serve as *instruments* for the individuals' choice of field of study.

As documented below, student choices are indeed interdependent. Specifically, we find that there is a high correlation between the ultimate education choices of seniors and the ultimate educational choice of the two years younger freshmen. There is a good reason why the interdependence should appear between seniors and first year students (rather than between seniors and second year students) during the period we study. This is the institutional setup of the Danish High-School system before 1990, which is discussed below.

We interpret these findings as reflecting the influence from student interaction about the attractiveness of various fields of study. That is, it reflects the consequences of informational updating. The type of information conveyed is unlikely to be about labor market earnings; raw labor market earnings are relatively easy to observe. However, it is considerably harder to assess the broader "quality-of-life" pay-off to a specific education. For instance, what is the associated status, work environment and so forth? We hypothesize that student interaction serves to convey this kind of information. In addition, we conjecture that students with (revealed or hypothesized) preferences for particular fields of study likely hold an informational advantage *within* their preferred area. Accordingly, if an individual is more exposed to a peer group with preferences for the human arts, the more likely it will be that new information about the "quality-of-life" aspects of a working life

with a human arts degree is brought forward. This new information may affect the educational choice.⁶

In sum, we argue that the high school specific fraction of seniors choosing an education within the human arts, is a viable instrument for the choice of which type of tertiary education individuals pursue. With this instrument in hand, we proceed to estimate the impact of choosing an education in the humanities using an IV-Probit model.

Our IV point estimates differ substantially from the conventional OLS counterparts. After instrumenting the wage difference between human arts majors and other majors is reduced to 0.3% and in fact slightly positive, but insignificant. Thus, we find *no significant difference* in the impact from the education choice on wages. Hence, we are led to the following conclusions: Relative cognitive abilities seem to have a substantial impact on wages, and comparative intellectual abilities do seem to matter for the choice of which education to pursue. However, it seems that the impact from the education *per se* on wages only depends on the field of study to a very limited extent.

Naturally, one may question our identification strategy. In particular, one could argue that the first stage correlation between the educational choices of different high school specific groups is simply picking up (unobserved) school quality in various dimensions. Since such quality differences may influence productivity and wages this reasoning would suggest that our instruments are invalid.

We believe that such concerns are unlikely in the present case for a number of reasons. First, Danish high schools are (generally) publicly funded, from a regional source. Hence, the type of local “neighborhood effects”, known to be operative in e.g. the US, whereby high income municipalities can provide better funding for educational facilities, are not operative in Denmark. Second, all Danish high schools follow the same curriculum, and all students attend the same (centrally devised) written exams. Third, in our analysis we are able to control for the identity of the high school, the individual have attended. If a specific high school happens to deliver high quality teaching in some particular field, a high school fixed effect picks it up. Finally, we show that the correlation between

⁶Note that since the hypothesis emphasizes information *updating*, it does not follow that more information about (say) the humanities necessarily will *increase* the probability that one would choose an education within this field of study. Section 6 contains a more detailed discussion of this issue.

the educational choice of a particular cohort and its contemporaries in their high school exhibit an “age effect”. For instance, there is *no* correlation between the ultimate educational choice of a given cohort and the ultimate educational choice of a one year *younger* cohort, a one year *older* cohort, or a four years *older* cohort. But there *is* an association between the ultimate educational choice of a cohort and the ultimate educational choice of a two and a three years *older* cohort. This general pattern is hard to explain away by appealing to high school specific (quality) effects. At the same time, it is consistent with peer effects running from older students to younger students, as explained below.

Another (and somewhat related) threat to the causal interpretation of the IV estimates is that our instrument is simply capturing student self-selection into high schools and, thus, systematic (unobserved) student ability variation across high schools. To evaluate this concern, we restrict our sample to students that i) live in the municipality where the high school is located and ii) live in municipalities with no more than one high school. We obtain findings that are very similar to those obtained with the full sample, implying that self-selection into high schools are unlikely to be responsible for our findings.

The paper proceeds as follows. The next section provides a brief review of the existing literature which estimates the return to different types of education. Section 3 presents the empirical strategy and section 4 describes the data and provides some institutional background on the Danish educational system. Section 5 presents the baseline OLS results. Section 6 presents the identification strategy and the IV results, while section 7 discusses potential threats to identification. Section 8 concludes.

2 Related Literature

While the literature on the return to schooling is vast, only a relatively limited number of studies have attempted to come to grips with the return to *type* of education.

James et al (1989) is the earliest contribution which provides evidence of differences in human capital remuneration by field of study. Specifically, they add dummy variables to an annual

earnings equation capturing college majors. Their sample includes earnings and various individual specific characteristics (including the college attended) of 1241 males, drawn from the *National longitudinal study of the high school class of 1972* (NLS72). They find very large differences in the “return” to college major. For instance, a student who chose his major in the humanities, instead of engineering, should expect 45% lower annual earnings in 1985, *ceteris paribus*; a truly remarkable return difference. Indeed, as James et al. concludes (p. 251): “[...] while sending your child to Harvard appears to be a good investment, sending him to your local state university to major in Engineering, to take lots of math, and preferably to attain a high GPA, is an even better investment.” On *a priori* grounds, however, their estimates may not reflect a causal impact on productivity for two reasons. First, their labor market data concerns annual earnings. As a result, some of the observed difference may be attributed to differences in number of hours worked in different occupations. Second, the choice of major is treated as exogenous.⁷

Blundell et al. (2000) draw on the UK *National Child Development Survey*, which contain data on family background of children born in 1958 (between March 3 and 9), their educational choice (including the subject studied) along with labor market data on hourly wages. The wage is observed for the year 1991, when the subjects were 33 years old. In contrast to previous studies, Blundell et al. (2000) also attempt to deal with the endogeneity problem by invoking matching methods to identify the impact from higher education on hourly wages. Specifically, individuals with a higher education were compared with individuals who could have taken a degree (based on previous educational performance) but chose not to, while sharing various observable characteristics (like ability, family background etc.).⁸ In line with previous studies, Blundell et al. also detect

⁷Daymont and Andrisani (1984) also contain information about fields of study; but their focus is on showing that the gender gap in wage regressions shrink, once the choice of major is accounted for. Other studies that investigate earnings differential across majors include Dolton and Makepeace (1990), Grogger and Eide (1995) and Loury and Garman (1995). A common feature of these studies is that they also (in contrast to the present study) treat the choice of type of education as exogenous.

⁸This approach is similar to the OLS wage regressions reported below; like the National Child Development Survey our data contain very rich socio-economic background information of the individuals pursuing a higher education, which we control for along side more standard variables like work experience etc. Naturally, this only resolves the endogeneity problem if all relevant individual specific characteristics are controlled for. If unobservable characteristics matter for wages and choice of education the estimates remain biased.

differences in labor market rewards across fields of study. For example, chemistry and biology exhibits the lowest return, whereas economics, accountancy and law the highest. In many cases, however, the effects from educational type are not very precisely estimated, presumably because of a rather limited sample size.

Bratti and Mancini (2003) also examine data from the UK. Like Blundell et al. (2000) they invoke matching methods. In addition, they also consider the problem that selection may take place over unobservable variables. In ensuring identification they rely on a multinomial-logit-OLS (MLO) set up, where choice of education is estimated and then the impact of education type on wages. As in the present paper they invoke an IV methodology. In Bratti and Mancini the exclusion restriction is that choices made in previous education (specifically: A-level curriculum) and the age of the student does not matter for wages directly, controlling for type of degree and standard Mincer-type controls. While their OLS results suggest that graduates from economics and business subject did better than the rest, their MLO results lead to no clear-cut ranking of subjects; the pecking order appears to change over time. One may argue, however, that their data is not optimal. The reason is that the data source (University Statistical Research data) does not include information about salaries. Since the authors do have access to fairly detailed information about occupation, they can construct salaries for individuals. This is done by using data from the New Earnings Survey; individual's salaries are computed as (p. 9) "*the average gross weekly pay of individuals employed full time (in the same occupation) in the year following the questioner*". Hence, by construction there is no within-occupation variation in earnings in their sample. As a result, their results are likely to speak to the impact from the type of education on occupations, rather than on wages *per se*; potentially valuable information pertaining to *differences* in wages across individuals with different educational backgrounds *in similar occupations* cannot be used for the purpose of identification.

Finally, Arcidiacono (2004) examine the return to college major, by modelling the educational decision explicitly. Arcidiacono, like James et al. (1989), rely in the NLS72 data set, implying the return estimates speak to earnings, rather than wages *per se*. The study documents that selection is

indeed taking place. Moreover, controlling for selection, Arcidiacono still finds considerable return differences across majors; as in James et al. students majoring in e.g. the natural sciences fare better in the labor market.

3 Empirical Strategy

In estimating the relative return to field of study we specify a wage equation that includes the individuals choice of educational type. The wage earned by individual i is denoted by y_i . S_i measures the education type (major in human arts or other types) chosen by individual i and is the binary endogenous variable of interest. S_i equals one for having a master's degree within human arts and zero otherwise; the return estimate of human arts is therefore *relative* to other majors. This indicator is used because we restrict our self to include tertiary educations of about equal duration, and because the Danish educational system is such that one specializes in one topic only at the university.

Our wage regression is

$$\log(y_i) = \alpha + \rho \cdot S_i + \mathbf{x}_i\boldsymbol{\beta} + d_{i,c} + d_{i,t} + d_{i,s} + u_i. \quad (1)$$

The parameter ρ captures the relative return on a degree within the humanities; it is the key parameter of interest. The vector \mathbf{x}_i consists of observed background variables to be described below; this set includes standard controls in wage regressions. The variables $d_{i,j}$ – $j = c, t, s$ – are various fixed effects which we introduce to try to control for ability; both the absolute level and the relative level. We expect these fixed effects to affect wages, and the choice of educational type, S_i .

The fixed effects are $d_{i,c}$ for high school curriculum, which should capture the individual's own assessment of the costs of acquiring specific skill types. We describe this variable in greater detail below. The variable $d_{i,t}$ controls for time effects. More precisely, this is the year of graduation from high school. Finally, $d_{i,s}$ is included to control for the attended high school and thereby potential quality differences in skills formation.

Ultimately we will treat S_i – the indicators for educational type – as endogenous. In order

to obtain consistent estimates for ρ we therefore employ a two-step IV procedure suggested by Wooldridge (2002). The first step involves estimating a probit model for the choice of educational type

$$P_i(S_i = 1 | \mathbf{x}_i, d_{i,c}, d_{i,t}, d_{i,s}, \mathbf{z}_i) = G_i(\mathbf{x}_i, d_{i,c}, d_{i,t}, d_{i,s}, \mathbf{z}_i; \gamma_i) \quad (2)$$

We estimate (2) using maximum likelihood, and the notation for the controls are the same as above. Hence, the *only* new entry is \mathbf{z}_i ; determinants of educational choice which do not matter to wages themselves. That is, our instruments for S_i . From a theoretical point of view, we consider variables which have an impact on the individuals expectations about the value of each type of education. Empirically, our instruments have to satisfy the two requirements that (A) they are orthogonal to u_i and (B) they are highly correlated with the choice of education type, S_i .

Having estimated equation (2) we subsequently obtain the fitted values from the regression, \hat{G}_i . The second step of our two-step IV approach involves estimating equation (1) using 2SLS with \mathbf{z}_i as the instrument. As we control for all the determinants of S_i , except for \mathbf{z}_i , this provides us with IV estimates for the relative return to a degree in humanities.

4 Data

The data we use in our empirical analysis is a data set covering the Danish population of individuals graduating from Danish high schools during the period 1981-1990. The data are administered and maintained by Statistics Denmark that has gathered the data from three administrative registers: the Integrated Database for Labor Market Research (IDA), the Danish Income Registry and the Danish Student Registry.

For each individual, we have complete data on educational and labor market histories along with detailed information on other socioeconomic characteristics. The educational data comprise detailed codes for the type of education attended (level, subject, and educational institution) and the year for completing the education. The labor market data contain the *hourly* wages; measured as the annual labor income divided by total hours worked.

4.1 Sampling of Data

In our main estimation sample we focus on individuals that satisfy the following two criteria: (i) graduated from high school between 1981 and 1990 and proceeded to obtain a master's degree and (ii) was wage-employed in 2000; in the main regressions below we use the wage rate in 2000 as dependent variable. These exclusions leave us with a main estimation sample that consists of 29,700 individuals.

We confine attention to high school graduates from the period 1981-1990 since this period was characterized by a particularly useful institutional setting, which allows us to proxy comparative intellectual abilities. After 1990 the high school system changes. We describe the nature of the institutional setting in some detail below.

Using 2000 as the “base year” is a choice made for practical reasons. The last high school cohort in our sample graduated in 1990. In Denmark it is not uncommon for students to take a sabbatical before beginning their university studies. Moreover, few students manage to complete their studies within the prescribed period of usually five years. Hence, in order to include all cohorts in the sample 2000 is a reasonable starting point.

To even out potential yearly fluctuations in wages, we also use the average wage over the period 1999-2001 as dependent variable. After all, the null is that the choice of education matters to permanent income; averaging should increase the signal-to-noise in the dependent variable. Still, using time averages is not critical to the results, which is documented by using alternative definitions of the dependent variable and samples.

4.2 Explanatory Variables

4.2.1 High School Information

Figure 1 show graphically how a student proceeded through the Danish educational system, from lower secondary school to tertiary education, during the period 1981-1990. Individuals usually enter the Danish high school immediately after completing lower secondary school, and graduates after three years.

When applying to a high school for admission, the student was required to specify an over-all track to follow: “mathematical” or “language”. After completing the first year, students then self-selected into various “branches”, available for each track, as illustrated in Figure 1. Under the math track students could choose between math/physics, math/natural sciences, math/social sciences, or math/music, while under the languages track students could choose between languages/social sciences, languages/music, modern languages, or classical languages.⁹ Hence, individuals were grouped into eight distinct branches. During this institutional arrangement the curriculum was determined after strictly defined course packages, implying that knowing the track and branch provide fairly precise information about the curriculum, the students completed.

<Figure 1 around here>

The information about which *branch* the individual pursued in high school appear in (1) and (2) as the curriculum fixed effect (i.e., $d_{i,c}$) to control for relative cognitive abilities directly. Hence, the basic idea is that the choice of “branch” provides information about the individual students’ *relative* abilities; a math/social science major was likely not quite as mathematically inclined as a math-physics student; at least the level of math taught was objectively speaking higher in the math/physics branch compared to the math/social science branch.

The “branch based system” was in place until 1990; from 1991 onwards students were given much greater autonomy with regards to course packages. Hence, the reason why we only sample high school graduates up until 1990 is precisely because it marks the end of the branch based system.

Eventually we do not have to rely on being able to fully control for relative ability, since we pursue an IV approach. However, as will be seen: branch choices hold considerable explanatory power vis-a-vis post university wages, suggesting that relative abilities across subjects indeed matter.

In order to control for “absolute ability”, we use the grade point average (GPA), which enters

⁹In the last years of the sample a few experimental branches was allowed; e.g., Math/English and Math/Chemistry. Only very few students pursued these branches; they are excluded from our sample.

into \mathbf{x}_i . The GPA is a weighted average of the grades at the final exam at each course. The quality of the courses as well as the GPA is comparable across high schools since all students within the same cohort face identical written exams; all exams and major written assignments are evaluated by the student’s own teacher as well as external examiners; high school teachers from other high schools. The external examiners are assigned by the Danish Ministry of Education.

Completed high school is a general admission requirement for tertiary educations, as suggested by Figure 1. We have information on which high schools individuals attended (149 in total). This information enters as the high school fixed effect, (i.e., $d_{i,s}$) and serve as controls for high school quality. Moreover, we have information on year of graduation from high school, which enters as the graduation year fixed effect, i.e., $d_{i,t}$. This dummy captures information on experience in equation (1).

4.2.2 Tertiary Education

As mentioned above, we focus exclusively on individuals who ultimately obtain a master’s degree. The reason is that we want to avoid any selection bias in our results due to the choice of education length. Moreover, we partition the type of tertiary education into two bins: human arts vs “others”.¹⁰ This information enters in the regressions as individual choice of education type, i.e., in S_i .

4.2.3 Other Explanatory Variables

We also apply individual information not related to high school attendance as explanatory variables, i.e., variables that enter in \mathbf{x}_i . These are gender and parental education. Gender is included to control for the gender wage gap in (1), whereas it enters (2) to control for gender differences in relative abilities or preferences.¹¹ Parental education is controlled for by including a set of indicators

¹⁰In Denmark, a tertiary education in humanities includes the following main disciplines: ancient and modern languages, literature, history, philosophy, religion and visual and performing arts. Academic disciplines such as psychology, anthropology, cultural studies and communication are considered as social sciences.

¹¹There is evidence to suggest a gender bias in the context of educational choice. In an influential study, Benbow and Stanley (1980) examined nearly 10,000 mathematically gifted boys and girls at the ages of 12 to 14. Their main empirical finding was a significant gender difference in mathematical reasoning in favor of boys as measured

for each parent regarding both the length and type of their education.

Table 1 displays selective descriptive statistics for the samples. The sampling unit is the individual, and the table presents the distribution on type of tertiary education, the distribution of students on high school branches, their high school grade point average, and their gender.

<Table 1 around here>

Some aspects of the data are worth remarking on. Almost 5/6 chose the math track in high school, while only 1/6 chose the language track; the largest high school branch is math/physics. Recall, these statistics are all conditional on progressing to a tertiary education and being wage employed for an unbroken period of three years. As regards subsequent choice of education type, other educational types attract the most students compared to human arts that attract 10.5% of the students. Moreover, almost 60% consist of men. The high school GPA is 8.8, which is above average as expected.¹²

5 Baseline OLS results

In Table 2, we report the results from the standard wage regression. That is, the endogeneity problem is ignored.

<Table 2 around here>

To recapitulate: These regressions are performed for persons with a tertiary education, who are wage-employed in 2000. In column 1 of Table 2, only indicators of the choice of education type are

by the SAT-M. This observed difference could not be ascribed to differential course-taking accounts. Moreover, 20 years later Benbow et al. (2000) revisited the sample and studied the educational and career outcomes of the students; they document a significant difference *in education choices*, with boys (now around 33) more likely to have chosen an education within the natural sciences; girls were more likely to pursue an education within the humanities. Admittedly, it seems hard to assess whether (and to what extent) these findings have a “genetic” or cultural origin. But either way it would appear that women are more partial to the humanities, compared to men. We detect a similar pattern below.

¹² A numerical grading system is used in Denmark. The possible grades were at the time: 0, 3, 5, 6, 7, 8, 9, 10, 11 and 13; 6 were the lowest passing grade, and 8 were given for the average performance.

included in order to study the raw wage differences between human art majors and other majors. The “raw” wage gaps reveal that human arts graduates have 23% ($\exp[-0.2580]-1$) lower wages than other graduates.

In columns 2-5 of Table 2, more information is gradually introduced into the log wage regression to study how the estimated wage difference changes. In column 2, we introduce a gender dummy in the regression that enters negatively and significantly with a parameter corresponding to women earning an average wage that is about 15% lower than the average wage for men. In column 3, we introduce high school GPA and find that the average wage increases by around 2% per grade point. Column 4 includes curriculum fixed effect or the choice of high school branch that proxies for relative talent. It is evident that those who studied at the math/physics branch in high school earned the highest wages compared to any other branch. A high school curriculum in classical languages or language/music led to the lowest wages that on average were about 11% lower than that of math/physics. In general, those who chose the language track tend to earn lower wages than those that chose the math track. This suggests that mathematical abilities are valued more in the labor market than linguistic abilities. Finally, column 5 includes all above mentioned explanatory variables. In addition, we also include dummies for graduation year from high school, information for education length and type of parents and high school fixed effects. High school fixed effects come in addition to, for example, the effect of parental education and may comprise, e.g., teacher quality etc.

Over-all, when control variables are progressively added we observe that the relative difference in returns across fields of study shrinks from -23% to -17%, but remains statistically (as well as economically) significant.

6 Identification and IV estimates

As already mentioned, an obvious criticism of the results presented above is that there may be unobserved factors that are correlated with both the choice of education type and wages, which will bias the OLS estimates. To deal with this concern, we employ an IV strategy based on the idea

that individuals' educational choice is influenced by that of older students in their high school. This section proceeds in three steps. First, we provide a simple theoretical argument which motivates our identification strategy. Next, we explain how our instrument is constructed. Finally, we discuss our baseline IV results.

6.1 The logic of the IV strategy: Theory

Consider an individual who is to decide which type of education to pursue. The individual derive utility from wage income, y , and “quality of life” more broadly, q . The latter variable is thought to capture, in a parsimonious way, factors such as status, work environment and job satisfaction associated with being employed using education of type $i = H(\text{uman arts}), O(\text{ther})$. Without loss we assume that wage income is observable, whereas q is something individuals hold expectations about. Utility is separable in the two arguments (y, q) , and the expected level of utility for an individual (the index of whom is suppressed in the interest of brevity) is therefore

$$E[U(y, q)] = u(y) + \int v(q) f(q) dq,$$

where $f(\cdot)$ is the density function for q .¹³

We assume $f(\cdot)$ supports a given variance σ^2 and mean μ ; both may be specific to either type of education: (μ_i, σ_i^2) , $i = H, O$. Importantly, both σ_i^2 and μ_i are thought to reflect the individuals' *perception* of the moments of the distribution of q . We treat both as known with subjective certainty in the derivations below, but both may vary from one individual to the next. In this sense we capture, in a simple way, differences in the information set of individuals at the time of optimization. Accordingly, these are the parameters which may be influenced by student-to-student interaction.

The felicity functions $u(\cdot)$ and $v(\cdot)$ exhibit positive and diminishing marginal utility: $u_y > 0$,

¹³See e.g. Fershtman et al. (1996) for an analysis of the allocation of talent in a society where individuals derive utility from consumption and social status. In the present case, however, we define “ q ” more broadly to include other aspects of final employment that individuals may value like work environment and job satisfaction.

$u_{yy} < 0$, $v_q > 0$, $v_{qq} < 0$. If we Taylor approximate v around the mean, μ , we obtain

$$v(q) \simeq v(\mu) + v_q(\mu)(q - \mu) + \frac{v_{qq}(\mu)}{2}(q - \mu)^2.$$

Evaluating expected utility we obtain, after some rearrangements, a simple representation of the preferences, which depends on income, expected quality of life and the variance of the latter¹⁴

$$E[U(y, q)] \approx u(y) + v(\mu) + \frac{v_{qq}(\mu)}{2}\sigma^2.$$

Now, suppose an individual with these preferences are to choose between two alternative types of education: H and O . Realistically, the individual undoubtedly will have different aptitude to the two forms of education. That is, different relative ability levels, which manifests itself in different wages. To capture this we may define the levels of income in final occupation as $y_H \equiv y(\alpha_H)$, and $y_O \equiv y(\alpha_O)$.¹⁵ The parameter α_i captures ability, and we expect the *relative* level of ability (α_H/α_O) to differ across individuals, reflecting variation in comparative cognitive ability. Hence, some students may have a comparative cognitive advantage in the humanities, implying $\alpha_H > \alpha_O \Rightarrow y_H > y_O$. For others, of course, it may be the other way round. The pertinent characteristic of α_i is that it is predetermined at the time of optimization; it may have been determined earlier in life, or simply at birth.

Next, one may suppose the perceived mean and variance of q in the two potential endeavours of life differ. For simplicity, suppose only the latter differs. If so the individual will prefer H to O iff

$$u([y(\alpha_H)]) + \frac{v_{qq}(\mu)}{2}\sigma_H^2 > u([y(\alpha_O)]) + \frac{v_{qq}(\mu)}{2}\sigma_O^2.$$

Hence, individuals with high ability in H will be more likely to choose this type of education. How-

¹⁴See the Appendix for derivations.

¹⁵Of course, we could easily admit wages to be affected explicitly by years of schooling etc. Say, by assuming $y_i = \alpha_i e^{\rho_i u_i}$, where ρ_i is the (potentially) field-specific return to a year of (field specific) education, u_i . Similarly, at the cost of some more notation, we could allow both dimensions of ability (α_H, α_O) to affect wages in either form of occupation; say $y_i = e^{\rho_i u_i} \Pi \alpha_i^{\beta_i}$. In general, then, we would allow the return to these abilities to differ; $\beta_H \neq \beta_O$. Finally, we abstract from “absolute” ability. This too could be introduced, perhaps defined as an average of the two components (α_H, α_O).

ever, greater uncertainty with respect to q (i.e., σ_H^2) may persuade the individual to do otherwise.

Accordingly, uncertainty about the non-pecuniary consequences of the educational choice may impact on what the individual decides, as a consequence of risk aversion. We hypothesize that *some* of the uncertainty may be resolved by interacting with fellow students. In particular, if the individual is exposed to students with information about q , this will lower σ^2 .

Naturally, the interaction could affect perceived μ as well. As a consequence of these multiple channels of influence, *the net impact* on the inequality from “more information” is ambiguous. For instance, if the result of the interaction is simply to lower σ_H^2 (say) then interaction should make it more likely that the individual chooses H . Alternatively, suppose the student-to-student interaction reveal information about μ . Naturally, if the information update implies $\mu'_H > \mu_H$ (with μ'_H being the revised mean), it should also make it more likely that the individuals chooses H . But if $\mu'_H < \mu_H$, the converse is true.

The key point, however, is that neither μ nor σ^2 matters to wages, y ; they only affect the educational choice. Accordingly, factors that lead to changes in (μ_i, σ_i^2) may be useful in identifying the impact of the educational choice itself. We hypothesize that student-to-student interaction, and thus the characteristics of the individuals’ peer group, may serve this purpose.

6.2 Constructing the Instrument

Our identification strategy is based on the idea that co-students influence the information set on which the individual base his or her final choice of a tertiary education. We do not doubt that individuals own abilities and interest are central. However, it would seem plausible that fellow students influence the individuals’ choice of education. This influence can take many subtle forms, including providing students with a sense of what a certain type of education implies in terms of job satisfaction given the individuals ability and interest. Such information could affect the individual’s expectations about the consequences of obtaining an education.

More concretely, we apply a measure of *older* student’s educational choice at the high-school as an instrument for the educational choice of younger students. The instrument is constructed as

follows: First, shares of individuals with tertiary education in humanities out of the total number of individuals with a tertiary education are constructed; the shares are determined for the group of individuals within the same high school and high school track. This implies that, two shares are calculated for each high-school per graduation year; one for the math track and one for the language track.¹⁶

Second, the shares are lagged *two* years, to capture the influence from seniors on freshmen. It is important to stress that seniors and freshmen (in Denmark they are two years apart) are *not* paired up arbitrarily. During the period we study students were to choose their academic specialization in high school *after* the first year.¹⁷ It seems plausible that high school specialization could give rise to a tendency to academic path dependence; early specialization affecting the ultimate form of specialization. Hence, if fellow students were to have a particularly strong impact on individuals choice of ultimate tertiary education, a major influence would be possible after one year of high school studies. This is the hypothesis we examine in the next section.

Before turning to the IV results, Table 3 presents summary statistics for the shares of students choosing human arts lagged two years. It is seen that the mean share of individuals with a tertiary education in humanities equals 0.22. The variable varies considerable from 0 for the 5 and 10 percentiles to about 2/3 for the 95 percentile. As described above, the variable is measured by high school, high school track, and graduation year, resulting in 2,252 clusters for the main sample of 29,700 individuals.¹⁸

<Table 3 around here>

¹⁶We calculate the instrument for each high school track separately because students had to choose track upon entry to high school.

¹⁷See Section 4.2.1.

¹⁸The main reason that there are only 2,252 clusters in the main sample (and not 2,980) is that not all of the 149 high schools exist over the entire period 1981-1990.

6.3 Baseline IV estimates

In this section, we present the IV results for the relative difference in returns across fields of study. The main specification applies the wage rate in 2000 as dependent variable and the squared value of the share of individuals graduating in human arts as instrument. We apply the latter variable as the main instrument because it implies the most clear-cut results, see the discussion below.

Table 4 reports our main IV results; the upper panel shows the second stage of the 2SLS regression, whereas the lower panel displays the results from the probit model: the probability model of the choice of education type. The results are estimated using clustering that allows for dependence in residuals within clusters.

<Table 4 around here>

Starting with the latter, the variable of particular interest is that from which we obtain identification: The square of the share of students two cohorts ago choosing human arts. As is clear from column 1 of Table 4, there is a statistically strong positive influence from the ultimate educational choice of the two years older cohort. This effect emerges despite the fact that we simultaneously control for gender, high school GPA, parental education, high school branch fixed effects, graduation year dummy's and high school fixed effects. Moreover, the instrument is significant and the χ^2 -test is about 14, suggesting that our instrument is not weak (Staiger and Stock, 1997).

Turning attention to the 2SLS estimate for the ultimate educational results, we observe a dramatic change: The point estimate is numerically very close to zero and in fact slightly positive, but is insignificant. This can be compared to the OLS results reported in column 5 of Table 2, where we found that a human arts education was associated with about 17% lower wages than other types of education.

As a result, we are led to the conclusion that the relative wage pattern observed in the (raw) data is primarily caused by selection into education types based on observed and especially unobserved relative ability. The fact that human arts majors earn much lower wages than the average acad-

emic employee is caused by the composition of their ability endowments and the returns to these endowments in the labor market rather than their field of study. Simply put, human arts majors are particularly negatively selected in terms of the market values of their ability endowments.

The standard error of the point estimate increases from 0.0060 to 0.0439; that is the standard error of the 2SLS is about 7 times larger than the OLS standard errors. In other words, the point estimate of the 2SLS regression is less precisely estimated than the point estimate of the OLS regression in Table 2. However, as is well-known, larger confidence intervals is a price we must pay to get a consistent estimate on the relative returns to education. It should be emphasized that the magnitude of the increase in standard errors is in line with those usually found in the literature on returns to schooling, see e.g. Angrist and Krueger (1991), Card (2001), and Fersterer, Pischke and Winter-Ebmer (2008). Finally, the endogeneity test of the indicator for educational type equals 19,518, which implies that the null hypothesis – that the indicator for educational type can be treated as exogenous – can be rejected at the 1% significance level.

In column 2-3, two additional regressions are presented. The results in column 2 are based on the average wage rate over the years 1999-2001 for the sample of 23,434 individuals that were employed in wage work during the three year period 1999-2001. It is seen that the stage two results are similar to those obtained in the main regression in column 1: The return to human arts majors and other majors are fairly similar. The probit regressions in column 1 and 2 are almost identical.

Finally, column 3 presents the IV results where the two-year lagged share of individuals graduating in human arts enters as instrument directly; not as the squared value. It is seen that the qualitative results of column 1 is supported, i.e., the instrument enters positively and significantly, and the point estimate of the 2SLS is numerically close to zero. We prefer the specification presented in column 1 – and thereby the squared value of the share of individuals educated in human arts as instrument – because the χ^2 -test from the probit model is higher in this specification.

7 Threats to Identification

Having established from our baseline IV estimates that the observed wage gap between human arts majors and other majors is primarily caused by selection into fields of study based on especially unobserved comparative advantages, we now examine two threats to identification: unobserved high school quality and self-selection into high schools.

7.1 Does the instrument capture high school quality?

Although we control for high school fixed effects, one may reasonably question whether our instrument is really capturing “peer effects”. An alternative interpretation could be that it captures some unobserved quality aspects of individual high schools.

For instance, it might well be the case that some schools have a stronger faculty in human arts courses than others, for which reason a larger fraction of a cohort eventually chooses human arts as their tertiary education. This is a dimension of high school quality that is not captured by high school fixed effects. If this constitutes a persistent effect (and it likely would be, of course), we would expect that the unobserved quality effects shows up as a (spurious) cross-cohort correlation in the ultimate educational choice. Worse, the underlying quality effects might influence wages directly, thereby rendering the instrument invalid.

Observe, however, that this interpretation of the probit findings in Table 4 suggest a time-invariant cross-cohort correlation. If what we are picking up is a quality fixed effect, we would expect to see that the partial correlation is relatively unaffected if we instead employed (say) the fraction of the students in a *younger* cohort, that eventually chooses human arts. But that is not what we find.

Figure 2 depicts the changes in the partial correlation between cohorts educational choice, when we modify the lag structure; the “lag 2” entry reproduces the result from Table 4. The interesting finding is that the partial correlation displays a distinct pattern. The correlation with *younger* cohorts (“lead”) is essentially nil. There is a positive correlation when we examine the educational choices of the one year older cohort (“lag 1”), however, the effect is insignificant. The maximal

effect is found when considering a two year older cohort (i.e., the educational choice of seniors). Lagging further leads to a gradually diminishing correlation; where a three year older cohort has a positive and significant effect, and a four year older cohort has a positive but insignificant effect.

<Figure 2 around here>

This pattern is no mystery if we consider the institutional setting, as described in Section 4.2.1. After the first year, recall, freshmen were to choose their area of specialization. Hence, this is the time where a potential influence from older students should be at its peak, which is consistent with the pattern depicted in Figure 2. The fact that there is a correlation with the three year older cohort is probably attributable to indirect effects. That is, the two year older students were affected by students that were older than them; an effect which might - partially - trickle down across the generations of students. It is seen that this effect is weakened the more lags we add. This explanation is reinforced by the fact that the correlation with the three year older cohort becomes insignificant when we estimate the probit model including only "lag 2" and "lag 3" (not reported).

Hence, while this pattern is consistent with the proposed hypothesis - involving cross-cohort informational spillovers - it is hard to explain by "quality effects". We view this check as a strong indication that our instrument is *not* picking up some unobserved high school specific quality effect.

7.2 Self-Selection

Another (somewhat related) concern is that the student body of high schools with graduates that proceed to study human arts might be systematically different from high schools where this is not the case. That is, perhaps our instrument is simply capturing student self-selection and thus systematic (unobserved) student ability variation across high schools.

It is important to understand the scope for such selection effects. Danish high school students would, as a rule, apply for a high school in their local municipality. Still, there were exceptions to this rule. Hence, self-selection effects could be at work in two instances: (i) in the (non conventional) situation where the student managed to get accepted to a high school in a neighbouring municipality,

(ii) in situations where there are more than one high school to choose from in the local municipality.

Table 5

In Table 5, we therefore examine our IV results for the following sub-samples of the base sample: In column (1), we require that students live in the municipality where the high school is located. In column (2), we apply the sample of column 1 and restrict it further to require that there is one and only one high schools in the municipality. Moreover, students that live in municipalities without high schools are included, while students that live in Copenhagen are excluded.

Students that live in municipalities without high schools are included in the sample of column 2 of Table 5. The reason is that the samples would otherwise be cut too drastically. Of course, the students belonging to this group have the opportunity to self-select them into high schools of their preferred choices. To limit the extent of self-selection, students attending high schools in the Copenhagen area are excluded, which might be a “special” area for a number of reasons, but especially because the range of alternative high schools, the individual students could select among is particular large.

Hence, in column 1 we are dealing with a group of students that did not apply to – or did not manage to get accepted into – a high school in a neighbouring municipality. In column 2, we are trying to limit situations where there are more than one high school to choose from in the local municipality. We are not able to handle this in a perfect manner, since the restrictions that we prefer results in a sample of 6,000 students only.¹⁹ This is why we include students that live in municipalities without high schools. We expect that this group of students to a large extent only have one high-school – the nearest – to apply to, especially when students outside the Copenhagen area are considered.

The point estimates from these specifications with alternative samples, columns 1-2 of Table 5, are quite similar to that in the base sample, although the sample size has been reduced by up to

¹⁹The restrictions for this sample are: (i) students attended high school in the municipality where they live and (ii) municipalities with one high school only.

50% of the base sample. The estimated return from a tertiary degree in human arts is about the same. This suggests self-selection into high schools is unlikely to be invalidating our identification strategy.

8 Conclusions

In this paper, we have examined the efficiency of human capital production across different types of education by exploiting Danish register data. If some fields of study are more efficient in producing human capital, this should manifest itself in a superior labor market performance of its graduates. Baseline OLS regressions reveal that students of human arts fare the worst in the Danish labor market with an hourly wage rate about 20% below that of graduates within other majors.

One may suspect, however, that the partial correlation between the type of education and wages does not convey accurate information about human capital production. If the selection into educational types is non-random, the OLS estimates will be biased. Our analysis confirms that selection seems to be at work. Socioeconomic circumstances, absolute ability, as well as relative cognitive abilities, measured by high school course work, influence the choice of education type.

Consequently, we invoke instruments for education type to address the selection problem. Our instrument is based on the influence from other students on individuals' choice of education type. Strikingly, once education type is instrumented, we find no statistically significant difference in the wage gap between human arts majors and other majors. This result suggests that a tertiary degree in humanities do not provide individuals with significantly less productive human capital than other types of tertiary education. Accordingly, the relatively poor wage performance of human arts majors in the Danish labor market is mainly due to selection according to relative cognitive ability, rather than to low human capital production at universities.

The present analysis raises new questions worth exploring in future research. First, wage differences seem to be related to relative cognitive abilities; mathematics appears to be important, for example. But why is that? Is it because such abilities are relatively scarce in the population or because they are particularly productive? If the latter is the case, then it would be useful to try

and discern why such abilities are in high demand. Further motivation for pursuing this question is found at the macro level where test scores in math and natural sciences seem to be a stronger linear predictor of aggregate growth than test scores for reading (Hanushek and Woessmann, 2009).

Second, how are relative abilities in, e.g., math and human science formed? As they determine both educational choices and wages, it would be useful to know whether these cognitive traits have a genetic origin, or are acquired during primary and secondary schooling. If the former is the case, education policies cannot be invoked to influence them; and conversely if relative talents are acquired.

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A Deriving Expected Utility

The second order Taylor approximation

$$v(q) \approx v(\mu) + v_q(\mu)(q - \mu) + \frac{v_{qq}(\mu)}{2}(q - \mu)^2$$

Observe that $(q - \mu)^2 = q^2 + \mu^2 - 2q\mu$. Hence

$$v(q) \simeq v(\mu) + v_q(\mu)(q - \mu) + \frac{v_{qq}(\mu)}{2}q^2 + \frac{v_{qq}(\mu)}{2}\mu^2 - \mu q v_{qq}(\mu)$$

Inserted into the utility function we obtain

$$\begin{aligned} E[U(y, q)] &\approx u(y) + v(\mu) + v_q(\mu) \int q f(q) dq - \mu v_q(\mu) \\ &\quad + \frac{v_{qq}(\mu)}{2} \int q^2 f(q) dq + \frac{v_{qq}(\mu)}{2} \mu^2 - \mu v_{qq}(\mu) \int q f(q) dq. \end{aligned}$$

A useful result regarding means and variances is that $\int q^2 f(q) dq = \mu^2 + \sigma^2$. Using it in the expression above we get

$$E[U(y, q)] \approx u(y) + v(\mu) + \frac{v_{qq}(\mu)}{2} \sigma^2$$

FIGURE 1

A Sketch of the Danish High School System, 1981-1990

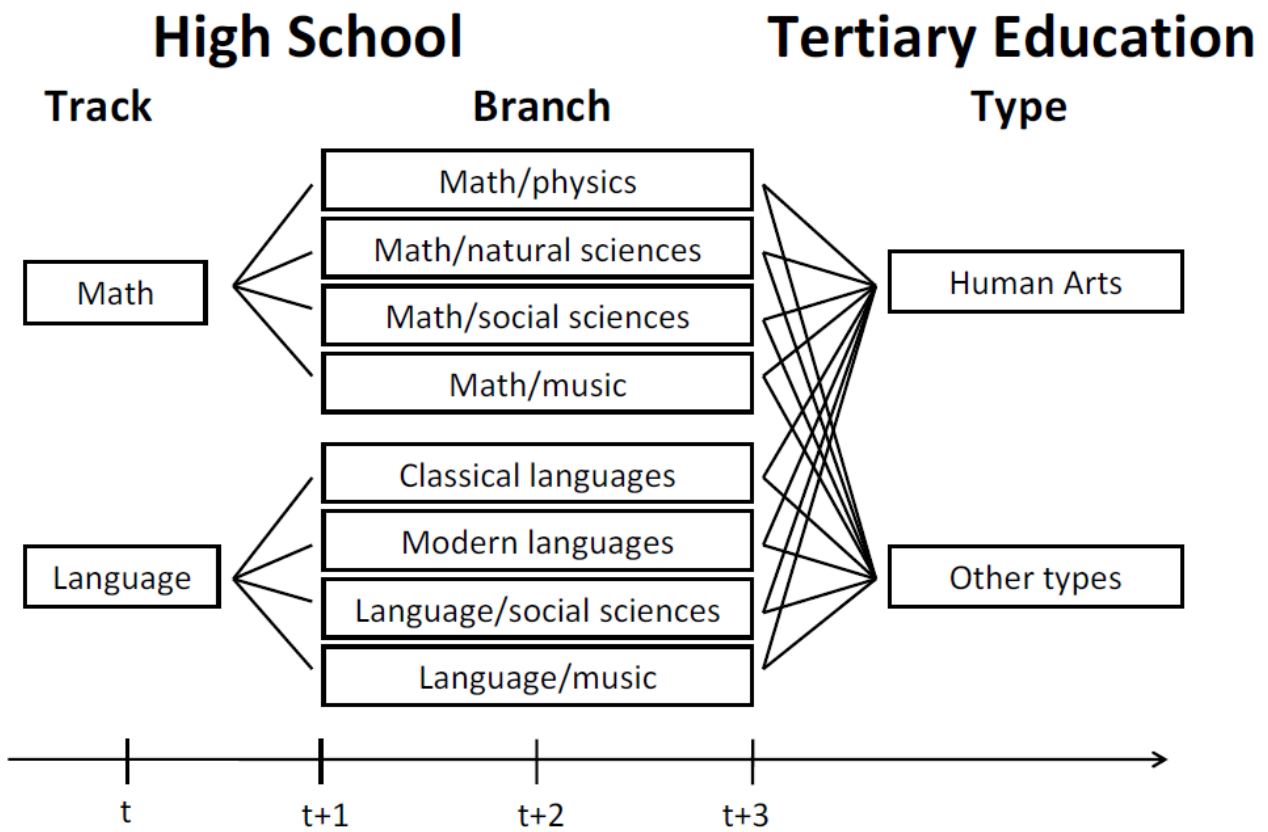
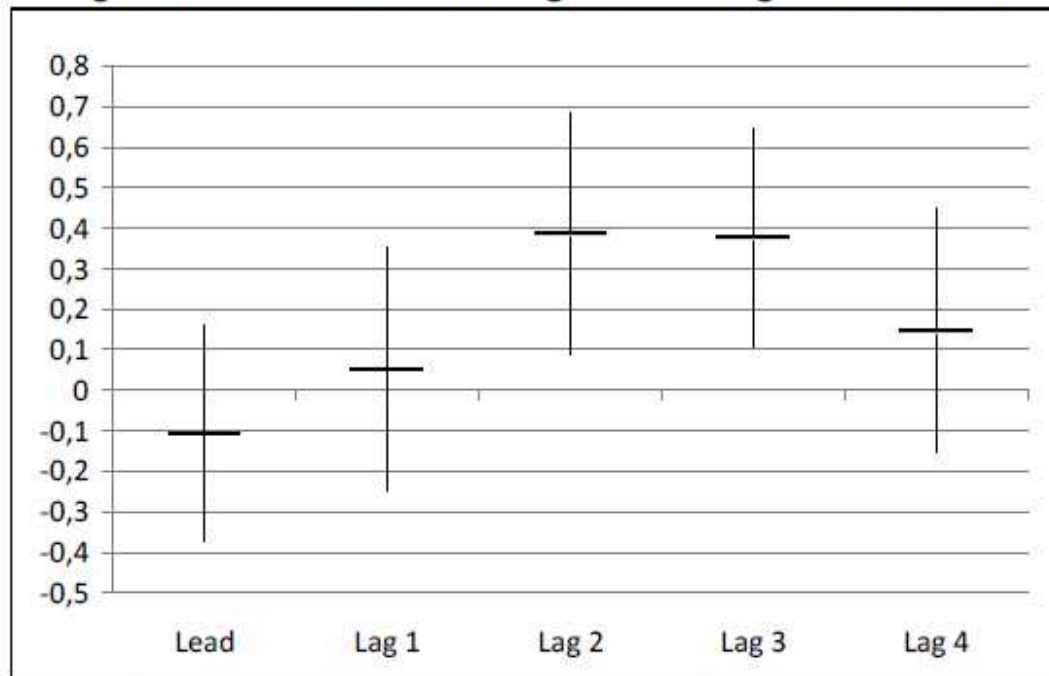


FIGURE 2
Significance of Instrument using Lead and Lags of Instrument



Notes: The figure presents point estimates (horizontal lines) and 95% confidence intervals (vertical lines) for 5 instruments in the probit regression based on the average hourly wage rate in 2000 as the dependent variable. The instruments is defined as the squares of the shares of graduates within human arts. The applied instruments are: the squared shares of one year younger cohort (lead 1), one year older cohort (lag 1), two years older cohort (lag 2), three years older cohort (lag 3), and four years older cohort (lag 4). The sample size equals 23.275 individuals.

TABLE 1
Summary Statistics for the Main Estimation Sample

	Mean	Std. Dev.
Log(wage rate)	5.436	0.322
<i>Subsequent Education Type</i>		
Human Arts Share	10.5%	
Other Educational Types	89.5%	
<i>High School Branch</i>		
Math-Music	5.9%	
Math-Physics	40.8%	
Math-Natural Sciences	20.8%	
Math-Social Sciences	15.9%	
Modern Languages	7.1%	
Classical Languages	0.4%	
Language-Social Sciences	7.6%	
Language-Music	1.4%	
High School GPA	8.824	0.8529
Women	41.10%	
Number of individuals	29,700	

TABLE 2
OLS Log Wage Regression Estimates

	(1)	(2)	(3)	(4)	(5)
Human Arts	-0.2580*** (0.0059)	-0.2242*** (0.0058)	-0.2241*** (0.0058)	-0.2054*** (0.0062)	-0.1890*** (0.0060)
Women		-0.1567*** (0.0037)	-0.1594*** (0.0036)	-0.1365*** (0.0037)	-0.1355*** (0.0036)
High School Grade			0.0208*** (0.0021)		0.0333*** (0.0020)
<i>High School Branch (ref: Math-Physics)</i>					
Math-Music				-0.1254*** (0.0077)	-0.0425*** (0.0081)
Math-Natural Sciences				-0.0886*** (0.0048)	-0.0856*** (0.0047)
Math-Social Sciences				-0.0610*** (0.0052)	-0.0332*** (0.0051)
Modern Languages				-0.0531*** (0.0076)	-0.0803*** (0.0074)
Classical Languages				-0.1111*** (0.0270)	-0.1364*** (0.0264)
Language-Social Sciences				-0.0952*** (0.0071)	-0.0868*** (0.0069)
Language-Music				-0.1103*** (0.0153)	-0.0919*** (0.0151)
Parental Education	NO	NO	NO	NO	YES
Graduation Year Fixed Effects	NO	NO	NO	NO	YES
High School Fixed Effects	NO	NO	NO	NO	YES
R-squared	0.0607	0.1172	0.1202	0.1342	0.2117
Number of Individuals	29,700	29,700	29,700	29,700	29,700

Notes: Standard errors clustered by high school, high school track and graduation year are reported in parentheses. * = significant at the 10% level, ** = significant at the 5% level, *** = significant at the 1% level. All specifications are estimated using OLS. The dependent variable in all specifications is the hourly wage rate in 2000. The independent variable of interest is "Human Arts" and the reported coefficients of this variable can be interpreted as the return to human arts majors relative to that of other majors.

TABLE 3
Summary Statistics for the Instrument

Fraction of Students Two Cohorts Ago Choosing Human Arts:

	Mean	Std. Dev.
Over-all	0.216	0.217
5th percentile	0.000	
10th percentile	0.000	
25th percentile	0.050	
50th percentile	0.121	
75th percentile	0.355	
90th percentile	0.556	
95th percentile	0.667	
Clusters	2,252	

TABLE 4
IV Estimates

2nd Stage of 2SLS	(1)	(2)	(3)
	Main	3-year avg. wages	Alternative instrument
Human Arts	0,0034 (0.0439)	-0,0123 (0.0464)	0,0059 (0.0436)
Women	-0.1419*** (0.0038)	-0.1383*** (0.0038)	-0.1420*** (0.0038)
High School GPA	0.0354*** (0.0023)	0.0336*** (0.0024)	0.0354*** (0.0023)
Probit model			
(Fraction of Students Two Cohorts Ago Choosing Human Arts) Squared	0.4636*** (0.1231)	0.4499*** (0.1393)	
(Fraction of Students Two Cohorts Ago Choosing Human Arts)			0.2998*** (0.1068)
Women	0.2199*** (0.0231)	0.1975*** (0.0272)	0.2195*** (0.0231)
High School GPA	-0.0689*** (0.0140)	-0.0493*** (0.0165)	-0.0692*** (0.0139)
Other Controls			
Parental Education	YES	YES	YES
High School Branch Fixed Effects	YES	YES	YES
Graduation Year Fixed Effects	YES	YES	YES
High School Fixed Effects	YES	YES	YES
Cluster-Robust Chi-squared-Test	14,18	10,43	7,88
Clusters	2,252	2,186	2,252
Share of Individuals with a Human Arts degree	10.5%	9.1%	10.5%
Number of Individuals	29,700	23,434	29,700
Endogeneity test	19,518 0.0000	14,276 0.0002	20,561 0.0000

Notes: Standard errors clustered by high school, high school track and graduation year are reported in parentheses. * = significant at the 10% level, ** = significant at the 5% level, *** = significant at the 1% level. All specifications are estimated using 2SLS with probit in the first stage regression. The dependent variable in column 1 and 3 is the hourly wage rate in 2000, while the dependent variable in column 2 is the average hourly wage rate over the years 1999-2001. The independent variable of interest is "Human Arts" and the reported coefficients of this variable can be interpreted as the return to human arts majors relative to that of other majors.

TABLE 5
Self-Selection

2nd Stage of 2SLS	(1)	(2)
Human Arts	-0,0361 (0.0498)	-0,0491 (0.0655)
Women	-0.1447*** (0.0049)	-0.1375*** (0.0056)
High School GPA	0.0350*** 0,0028	0.0343*** (0.0031)
Probit regression		
(Fraction of Students Two Cohorts Ago Choosing Human Arts) Squared	0.5434*** (0.1580)	0.5285*** (0.1575)
Women	0.2252*** (0.0290)	0.2331*** (0.0334)
High School Grade	-0.0742*** (0.0175)	-0.0255 (0.0196)
Other Controls		
Parental Education	YES	YES
High School Branch Fixed Effects	YES	YES
Graduation Year Fixed Effects	YES	YES
High School Fixed Effects	YES	YES
Cluster-Robust Chi-squared-Test	11,83	11,25
Clusters	2.117	1.546
Number of Individuals	18.579	14.514
Endogeneity test	9,232 0.0024	5,024 0.025

Notes: Standard errors clustered by high school, high school track and graduation year are reported in parentheses. * = significant at the 10% level, ** = significant at the 5% level, *** = significant at the 1% level. All specifications are estimated using 2SLS with probit in the first stage regression. The dependent variable in all specifications is the hourly wage rate in 2000. The independent variable of interest is "Human Arts" and the reported coefficients of this variable can be interpreted as the return to human arts majors relative to that of other majors. Column (1) includes students that live in the municipality where the high school is located. Column (2) includes students that live in the municipality where the high school is located and with only one high schools in the municipality. Moreover, students that live in municipalities without high schools are included, while students that attend a high school in the Copenhagen area are excluded.

Chapter 3

Estimating Taxable Income Responses Using Danish Tax Reforms

Henrik Jacobsen Kleven

Esben Anton Schultz

Estimating Taxable Income Responses using Danish Tax Reforms*

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November 2011

Abstract

This paper presents evidence on taxable income responses using administrative data that link tax return information to detailed socioeconomic information for the entire Danish population over 25 years. The identifying variation is provided by a series of tax reforms that create large tax variation across individuals, income forms, and over time. It is argued that the unique tax variation and data in Denmark makes it possible to control for the biases from non-tax changes in the income distribution and mean reversion that plague much of the existing literature. Our main findings are the following: *(i)* Labor income elasticities are modest overall, around 0.05 for wage earners and 0.10 for self-employed individuals. *(ii)* Capital income elasticities are about 2-3 times larger than labor income elasticities. *(iii)* Behavioral elasticities are much larger when estimated from large tax reform episodes than for small tax reform episodes, consistent with idea that responses to small tax changes are attenuated by optimization frictions such as adjustment costs and inattention. *(iv)* Cross-tax effects between labor and capital income—for example due to income shifting—are in general small. *(v)* All of our findings are extremely robust to specification (such as pre-reform income controls), suggesting that we have controlled in a sufficiently rich way for non-tax factors impacting on taxable income.

JEL Classification Codes: H24; H31; J22.

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1 Introduction

The recent literature on behavioral responses to taxation has shifted its focus from the elasticity of hours worked to the elasticity of reported taxable income. Effects on taxable income capture the full range of responses to taxation, including hours worked, unobserved effort, training, occupational choice, tax avoidance and tax evasion, and therefore provides a more complete picture of the behavioral response to taxation. Moreover, as argued by Feldstein (1995, 1999) in two influential papers, the elasticity of taxable income provides a sufficient statistic for the revenue and efficiency effect of income taxation, which places this parameter at the centre stage of all the major normative questions in public finance such as the structure of optimal income redistribution and the size of government.

A large and growing literature estimates the elasticity of taxable income using tax return data. Most of this work is based on the United States and uses as its source of identification a series of tax reforms in the 1980s and 1990s that were associated with substantial tax changes at the top of the income distribution (e.g. Feldstein 1995; Auten and Carroll 1999; Moffitt and Wilhelm 2000; Goolsbee 2000; Gruber and Saez 2002; Kopczuk 2005; Giertz 2007). In addition to the U.S. literature, a number of recent studies estimate taxable income responses in other countries that have lowered marginal tax rates at the top of the income distribution through the 1980s and 1990s, including the United Kingdom under the Thatcher administration (Brewer, Saez, and Shephard 2010), Canada (Sillamaa and Veall 2001; Saez and Veall 2005), Norway (Aarbu and Thoresen 2001), and Sweden (e.g. Hansson 2007; Blomquist and Selin 2010; Gelber 2010).¹

Reforms that strongly target the top of the income distribution provide interesting variation, but are also associated with some important empirical difficulties. Because the allocation of tax treatments is determined by pre-reform income level, we have to consider the possibility that different income groups differ in a number of non-tax dimensions that impact on taxable income

¹Alongside the large literature using income tax reforms to estimate taxable income responses, a recent smaller literature estimates taxable income responses using bunching around kink points created by discrete jumps in the marginal tax rate in piecewise linear income tax schedules (Saez 2010; Kleven et al. 2010; Chetty et al. 2011). While bunching around kink points provides a very compelling source of identification in principle, an important limitation of this strategy is that there tends to be very little bunching in empirical distributions. Only very sharp and salient kinks create any bunching at all, and even there the response is modest (Chetty et al. 2011). The likely explanation is the presence of optimization frictions associated with locating exactly at the cutoff (such as adjustment costs, misperception and inattention) combined with the fact that the utility gain of bunching in response to jumps in marginal tax rates is typically not very large (Chetty 2011).

and are correlated with the tax law changes. This problem is reinforced by the fact that tax return data typically contains very little information about tax payers besides income variables and tax rates, making it difficult to control for any non-tax differences across different tax payers.

Two specific problems have been discussed extensively in the literature (e.g. Slemrod 1998; Saez 2004; Saez, Slemrod, and Gieritz 2010). First, it is very hard to disentangle tax-driven increases in top incomes from increases that are driven by non-tax factors such as skill-biased technical change and globalization. This problem is particularly important in countries that have experienced strong secular increases in top income shares, and may result in a substantial upward bias in the elasticity estimates. Second, defining treatments and controls according to pre-reform income level creates a mean-reversion problem, because a tax payer with a positive income shock in the pre-reform year will tend to have a lower income in the following years, independently of the reform. For tax cuts at the top, this biases elasticity estimates downwards. In order to correct for the two biases mentioned above, the literature has attempted to control in a number of ways for pre-reform income levels. However, the richness of such income controls is constrained by the fact that the identification comes from different tax changes across pre-reform income levels, and in general the results turn out to be extremely sensitive to specification.

This paper presents new evidence on taxable income responses based on a series of Danish tax reforms and a very rich administrative dataset covering the entire Danish population over 25 years (1980-2005). The Danish tax context and data holds the promise to avoid the biases discussed above for the following three reasons. First, by linking tax return data to administrative data containing detailed socioeconomic information for every taxpayer, we control directly for some of the underlying non-tax components of permanent and transitory income that are important for the biases discussed above. Second, the evolution of the Danish income distribution has been much more stable than in most other countries, even compared to other egalitarian societies such as Sweden.² To illustrate this, Figure 1 shows top income shares based on a broad income measure including all labor income and capital income. As the figure makes clear, top income shares have been roughly constant over time with the exception of a tiny increase after the mid-1990s. The unchanging income distribution in Denmark isolates mean-reversion as the potential bias that must be controlled for.

² Atkinson, Piketty and Saez (2011) provide international overviews of the evolution of top income shares, while Roine and Waldenström (2008) study the Swedish experience.

Third, we consider a time period that encompasses a series of tax reforms, which create large and compelling identifying variation. In some years, the tax variation created by the Danish reforms is larger than the variation created by the major U.S. reforms of the 1980s, and importantly the Danish variation does not feature the same strong correlation with income level as the U.S. variation. The Danish reforms were associated with three main changes: (i) differential changes in marginal tax rates across different tax brackets, (ii) changes in bracket cutoffs that moved large groups of taxpayers to different brackets, and (iii) a move from a fully symmetric treatment of different income components (e.g. labor income vs. capital income and positive income vs. negative income) to an asymmetric treatment of different income components. The combination of points (i) and (ii) create large and nonlinear tax variation through the income distribution in a way that is not systematically correlated with income level. Point (iii) implies that income composition, besides income level, plays a key role for the tax bill, thereby creating variation across individuals at the same income level. All three changes together therefore create very rich identifying tax variation.

In a tax system based on asymmetric tax treatment of different income forms, estimating the elasticity of *overall* taxable income with respect to the marginal tax rate is not very meaningful as there is no single well-defined marginal tax rate associated with this income concept. More importantly, the presence of independent tax variation across different income forms provides a rare opportunity to analyze the anatomy of taxable income responses, a question which is interesting in its own right (Slemrod 1995, 1996, 1998; Saez, Slemrod, and Giertz 2010). To explore this, we estimate responses for the underlying components of taxable income separately, and we also consider specifications allowing for cross-tax effects between different income forms.

Our main findings are the following. First, labor income elasticities are modest seen over the full time period, around 0.05 for wage earners and 0.10 for self-employed individuals. Second, capital income elasticities are about 2-3 times larger than labor income elasticities. Third, behavioral elasticities (both labor and capital) are substantially larger when estimated from large tax reform episodes than from small tax reform episodes. This finding is consistent with the argument by Chetty et al. (2011) and Chetty (2011) that elasticities estimated from small tax changes are attenuated by optimization frictions (e.g. adjustment costs and inattention), whereas large tax changes are likely to overcome such frictions and hence reveal the true long-run elasticity. We find that a particularly large and salient tax reform in the late 1980s is associated

with a population-wide elasticity of labor income of about 0.12, whereas a series of smaller tax reforms in the 1990s and 2000s are associated with a labor income elasticity of only 0.02. Fourth, cross-tax effects between labor and capital income are in general weak, with a small degree of complementarity between the two income forms for wage earners and a small degree of substitutability for self-employed individuals. Since income shifting for tax avoidance purposes by itself would imply substitutability, our results suggest that income shifting is more prevalent for the self-employed than for wage earners.

Finally, we show that the above set of findings is extremely robust to empirical specification, including the specification of pre-reform income controls. We explore the different income controls that have been proposed in the literature along with alternative specifications that control in a richer way for mean reversion. The robustness of our findings is reassuring and suggests that we have controlled in a sufficiently rich way for non-tax factors impacting on taxable income. Against this background, we argue that the Danish context offers a useful laboratory allowing for a credible identification of taxable income responses.

The paper proceeds as follows. Section 2 describes the Danish tax system and tax reforms used to estimate behavioral responses, while section 3 describes the data. Section 4 describes the empirical specification and identification strategy. Section 5 presents the empirical results, while section 6 concludes.

2 The Danish Tax System and Tax Reforms

The Danish individual income tax system treats different income forms in a partially separate fashion, as opposed to standard tax systems that apply a progressive rate structure to a single measure of taxable income. The income concepts of the Danish income tax system, shown in Table 1, are given by labor income (LI), personal income ($PI = LI + \text{other PI}$), capital income exclusive of stock income (CI), stock income (SI), deductions (D), and taxable income ($TI = PI + CI + SI - D$). These income concepts are aggregated into several different tax bases that are taxed at different rates. The definition of those bases as well as the associated tax rates have undergone substantial changes over time due to a series of tax reforms, and this is the variation that we exploit to estimate behavioral elasticities.

Taxes are divided into national taxes and regional taxes at the municipal and county level,

but the two types of taxes are enforced and administered in an integrated system. At the national level, a series of important tax acts have been implemented over the past 25 years. Those tax acts are the 1987-reform, the 1994-reform, the 1999-reform (called the “Pentecost Package”), and the 2004-reform (called the “Spring Package”). Most of these reforms were phased in over several years, which generates considerable tax variation in most years of the period we consider. We also exploit changes in tax schedules at the regional level, but those changes have been much smaller and are more uniform across taxpayers than the national changes.

Throughout the period we consider, the national income tax has been divided into three main brackets: a bottom bracket, a middle bracket, and a top bracket. The past 25 years of tax reform have been associated with three main changes. First, a lowering of marginal tax rates in each bracket, with larger cuts in the middle and top brackets than in the bottom bracket. Second, a substantial broadening of the tax base as negative capital income and deductions were prevented from offsetting positive income on a one-to-one basis. This change was implemented by changing the tax schedule from a function of total taxable income (TI) to a function of each of the underlying income components (LI, PI, CI, SI, D), with a higher marginal tax rate on labor income than on the other income components as well as a higher marginal tax on positive income than on negative income (such as mortgage interest and deductions). With the exception of stock income, the taxation of the different income components is not fully separate and cross-effects in the tax function are therefore non-zero. Third, adjustments of bracket cutoffs that did not correspond to the base broadening, thereby pushing tax payers into higher brackets. This bracket push combined with the fact that tax rates were reduced within each bracket imply substantial and very heterogeneous tax rate variation through the income distribution. All of the changes together create strong variation across taxpayers at different income levels, across tax payers at similar income levels (but different income compositions), and across different income types. Below we describe the tax reforms in greater detail.

Table 2 shows the different tax rates and associated tax bases in four specific years: 1986 (before the 1987-reform), 1993 (before 1994-reform), 1998 (before the 1999-reform), and 2005 (after the 1999- and 2004-reforms). The tax system consists of a flat regional tax (shown for the average municipality) along with progressive national taxes levied on varying tax bases. The main national taxes are the bottom tax, the middle tax and the top tax, and in some years those taxes are supplemented by social security contributions, labor market contributions, and

an EITC. The different tax rates shown in the table are cumulative such that a taxpayer in the top bracket is subject to the sum of the bottom, middle, and top taxes (along with the other flat taxes).³ The table shows the tax base changes mentioned above. In the mid-1980s, all tax rates applied to overall taxable income, whereas in the 1990s and 2000s no tax rate applies to this broad income measure. In 2005, for example, tax liability is calculated from four different tax bases: taxable income exclusive of stock income ($PI + CI - D$), personal income plus positive net capital income ($PI + [CI > 0]$), labor income (LI), and stock income (SI).

There are two points to make with regard to those tax base changes. First, in a situation where taxable income include subcomponents that are treated differently, it is not meaningful to estimate an elasticity of overall taxable income with respect to the marginal tax rate as there exists no single, well-defined marginal tax rate for this income concept. We therefore consider the underlying income components of the tax system separately, focusing on labor income and capital income (excluding stock income). We do not consider stock income as it is taxed on a completely separate schedule, which has remained relatively constant throughout most of the period and therefore offers less variation than the rest of the income tax code.⁴ Second, the type of base broadening described above does not raise the conceptual problems that have been discussed extensively in the literature on taxable income responses (Slemrod 1998; Kopczuk 2005). The usual problem is that such reforms require us to consider constant-definition tax bases in order to avoid confounding behavioral and definitional changes, but in so doing we are relating the tax rate to an artificial tax base different from the one in the tax code in a given year. However, the base broadening shown in Table 2 does not pose this problem (and indeed create a lot of interesting variation that we will exploit), because it does not consist in including previously untaxed components in the tax system. In all years, the tax system depends on the same underlying income components, and it is instead the aggregation of those income components into tax bases that changes over time.

³For example, in 1986, a taxpayer in the top bracket would face a marginal tax rate equal to $28.1 + 14.4 + 14.4 + 10.8 + 5.5 = 73.2\%$. However, a marginal tax ceiling is in place in all years, and this ceiling equals 73% in 1986 and is therefore binding for a taxpayer living in an average municipality. In 2005, the marginal tax ceiling has dropped to 59.0% and was indeed also binding for a taxpayer in the top bracket living in the average municipality. For labor income, there is a labor market contribution of 8% on top of the tax ceiling, but at the same time labor income enters all the other tax bases net of the labor market contribution. The effective tax ceiling on labor income in 2005 is therefore equal to $8.0 + (1 - 0.08) \times 59.0 = 62.3\%$.

⁴The most useful quasi-experimental variation in stock income taxation is created, not by tax reforms, but by a sharp kink at the cutoff between two brackets in the stock income tax. Kleven et al. (2010) use bunching around this kink to estimate the elasticity of stock income and find evidence of strong behavioral responses.

Figure 2 illustrates the implications of the tax rate and tax base changes described above for the effective marginal tax rates on labor and capital income in each bracket (bottom, middle, and top) over time. For labor income (Panel A), the marginal tax rate in the top bracket has been declining from 73% to 62%, while the tax rate in the middle bracket has been declining from 62% to 49%. On the other hand, the bottom tax rate is increasing over the early part of the period and then declining over the later part of the period. Overall, the difference between the bottom tax and the middle/top taxes has been shrinking over this period, although the relative changes have not been dramatic. However, these graphs do not reveal the important implications of bracket push as we come back to below.

For capital income, we distinguish between negative capital income (Panel B) and positive capital income (Panel C) as the two are taxed very differently. For negative capital income, the three brackets have collapsed into one bracket subject to the bottom tax rate (as negative capital income was excluded from the middle and top tax bases). For taxpayers in the top bracket, the marginal tax rate associated with negative capital income has dropped from about 73% to 33% over the period, while for taxpayers at the bottom the drop has been much smaller. These dramatic tax changes affect a very large number of taxpayers, because capital income is in fact negative for the majority of Danish taxpayers as a result of interest payments on loans (mortgage and other loans). For positive capital income, we also see very large changes as the band between the top and the bottom first narrows substantially (since all capital income is excluded from the top tax base) and then widens substantially (since positive capital income is reintroduced in the top tax base).

Finally, to see the importance of bracket push due to under-adjustment of bracket cutoffs as bases were broadened, Panel D shows the evolution over time in the share of taxpayers located in each bracket. We see that the share of taxpayers liable to pay the top tax has increased dramatically from less than 10% of the population in the mid-1980s to almost 30% of the population in the mid-2000s. The share of individuals in the middle bracket has fallen from about 40% to slightly above 20% over the whole period, while the share of taxpayers in the bottom bracket falls from about 50% to 40% in the early part of the period and then rises back to 50% in the latter part of the period.⁵ These movements across brackets create substantial

⁵The bottom, middle, and top bracket shares do not quite add up to 1, because a small amount of taxpayers below a basic exemption level are not liable to pay the bottom tax.

tax variation, especially for labor income. The combination of the tax rate changes for labor income in Panel A and the bracket push in Panel D create very strong and heterogeneous tax variation through the income distribution.

Overall, the reforms described in this section implies substantial tax variation over time and across individuals. Indeed, as we show in section 4 when discussing the identification strategy, the variation in some years is comparable to the major tax acts in the U.S. in the 1980s and the Tax Reform of the Century in Sweden in the early 1990s.

3 Data

The analysis is based on a very rich panel that runs from 1980 to 2005 and covers the entire universe of Danish tax payers. The data set has been constructed by Statistics Denmark based on a number of administrative registers, including the Income Tax Register and the Integrated Database for Labor Market Research (IDA). For each individual, the data set contains detailed tax return information along with a large set of socioeconomic variables such as place of residence, gender, age, marital status, number and age of kids, immigrant status, ethnicity, employment status, job experience, education, occupation, and industry.

Marginal tax rates are not directly observed in tax return data, and we therefore have to simulate the marginal tax rate for each taxpayer based on tax return information and a model of the Danish tax system. As there exists no publicly available TAXSIM model for Denmark, we have constructed our own TAXSIM model of the Danish tax system over the period 1984-2005. Based on this model and tax return data, we compute the marginal tax rate on a given income component by increasing income by DKK 100 (\simeq USD 20). In particular, if tax liability $T(\cdot)$ is a function of n different income components z^1, \dots, z^n , we compute the marginal tax on z^j as $\tau^j = [T(z^1, \dots, z^j + 100, \dots, z^n) - T(z^1, \dots, z^j, \dots, z^n)] / 100$.⁶

Following Gruber and Saez (2002), the empirical strategy is to relate changes in reported income over time to changes in marginal tax rates over time for individual taxpayers. We focus on the period 1984-2005 and consider three-year intervals (1984-1987, ..., 2002-2005). We denote the first year in any given three-year interval by s and the last year by $s + 3$. We include only

⁶While the Danish income tax system is based on individual filing for married couples, it involves certain elements of jointness due to the fact that some exemptions can be transferred across spouses. This implies that, for a married person, income tax liability depends on both individual incomes and on spousal incomes. Our TAXSIM model fully accounts for this jointness.

tax payers that are also observed in year $s - 1$ and $s - 2$, because we use those years to construct pre-reform income controls. The three-year differences are stacked to obtain a dataset with about 49 million observations.

We restrict the sample used for estimation in the following ways. First, we restrict the sample to individuals aged 15-70 years. Second, we exclude individuals whose income in base year s comes primarily from welfare benefits, because this would require us to account for the important incentive effects of the welfare system and model extensive responses. Third, we limit our sample to people who are fully tax liable in Denmark. These restrictions leave us with a sample of about 37 million observations. Table 3 shows summary statistics for the estimation sample.

4 Empirical Strategy

4.1 The Model

The economic model underlying the new tax responsiveness literature is a simple extension of the traditional labor supply model. It is assumed that each tax payer maximizes a utility function of the form $u(c, z, x)$, where c is consumption, z is reported taxable income, and x is a vector of individual characteristics. We may think of reported income z as being generated by a number of underlying choices such as hours worked, unobserved effort, training, occupational choice, tax sheltering activities, etc. The implicit assumption in the literature is therefore that all those underlying activities are weakly separable from consumption in the utility function. Utility is maximized subject to a budget constraint $c = z - T(z) = (1 - \tau) \cdot z + y$, where $T(\cdot)$ is tax liability, $\tau \equiv T'(\cdot)$ is the marginal tax rate, and $y \equiv \tau \cdot z - T(z)$ is virtual income. We may then write the optimal choice of taxable income as $z = z(1 - \tau, y, x)$.

Consistent with the Danish setting described earlier, we extend the above model to account for the presence of multiple income types that are taxed differently. Consider therefore a consumer choosing incomes z^1, \dots, z^n under a tax schedule $T(z^1, \dots, z^n)$. This consumer maximizes utility

$$u = u(c, z^1, \dots, z^n, x), \quad (1)$$

subject to a budget constraint

$$c = \sum_{j=1}^n z^j - T(z^1, \dots, z^n) = \sum_{j=1}^n (1 - \tau^j) z^j + y, \quad (2)$$

where $\tau^j \equiv \partial T / \partial z^j$ is the marginal tax rate on income type z^j (which is in general a function of all the different incomes z^1, \dots, z^n) and $y \equiv \sum_{j=1}^n \tau^j z^j - T(z^1, \dots, z^n)$ is virtual income. Our measure of virtual income is a generalization of standard virtual income to a situation with multi-dimensional income. In the case where component z^j reflects deductions in taxable income, because z -variables are defined as income in eq. (2), this component would be equal to minus deductions.

In this model, the optimal choice of any given income type z^j depends on all the net-of-tax prices and virtual income, i.e.

$$z^j = z^j(1 - \tau^1, \dots, 1 - \tau^n, y, x). \quad (3)$$

In general, an empirical specification for a given income type z^j should account for both own-price effects of changes in the marginal net-of-tax rate on income j as well as cross-price effects of changes in the net-of-tax rates on all the other income types. Indeed, the Danish tax reforms offer a rare opportunity to identify cross-tax effects between different income forms. In the empirical analysis, we start by considering baseline specifications without cross-tax effects, and then turn to specifications that allow for cross-tax effects. The analysis of cross-tax effects enables us to evaluate the potential importance of income shifting between labor and capital income, an issue that has been much discussed in the literature.

In the baseline model without cross-tax effects, expression (3) implies $z_{is}^j = z^j(1 - \tau_{is}^j, y_{is}, x_{is})$ for tax payer i at time s . Adopting a log-linear specification (as is standard in the literature), we consider the following specification

$$\log(z_{is}^j) = \alpha + \varepsilon \cdot \log(1 - \tau_{is}^j) + \eta \cdot \log(y_{is}) + \gamma_s^c \cdot x_i^c + \gamma^v \cdot x_{is}^v + \mu_i + \nu_{is}. \quad (4)$$

In this specification, we distinguish between time-invariant individual characteristics x_i^c whose effect may change over time and time-variant individual characteristics x_{is}^v whose effect is constant over time. The effect of time-invariant individual characteristics whose effect is constant over time is subsumed in the individual fixed effect μ_i . The key variables of interest are the uncompensated elasticity of reported income (ε) and the income elasticity (η).⁷

⁷The specification of the income effect in terms of virtual income (as defined above) is different from the specification in several previous studies on taxable income elasticities (e.g. Gruber and Saez 2002; Kopczuk 2005), which specifies the income effect simply in terms of after-tax income $z - T(z)$.

In first-differenced form, the model can be written as

$$\Delta \log \left(z_{is}^j \right) = \varepsilon \cdot \Delta \log \left(1 - \tau_{is}^j \right) + \eta \cdot \Delta \log \left(y_{is} \right) + \Delta \gamma_s^c \cdot x_i^c + \gamma^v \cdot \Delta x_{is}^v + \Delta \nu_{is}. \quad (5)$$

In the baseline specification, differences at time s are three-year differences from s to $s + 3$.

4.2 Identification and Relationship to Previous Literature

Because of the nonlinearity of the tax system, the marginal tax rate and virtual income are endogenous to the choice of taxable income, which creates a correlation between $\Delta \log \left(1 - \tau_s^j \right)$, $\Delta \log \left(y_s \right)$, and the error term. The usual way to construct instruments for these variables is to use mechanical tax changes driven by changes in tax laws. Hence, using the Danish TAXSIM model described above, we simulate post-reform marginal tax rates under pre-reform behavior, $\tau_{s+3}^j \left(z_s^1, \dots, z_s^n \right)$, where we account for the fact that the marginal tax rate on income j may depend not just on the level of income j but also on the levels of the other incomes. From the simulated marginal tax rates, we obtain mechanical net-of-tax rate changes, $\log \left(1 - \tau_{s+3}^j \left(z_s^1, \dots, z_s^n \right) \right) - \log \left(1 - \tau_s^j \left(z_s^1, \dots, z_s^n \right) \right)$, which are used as instruments for the observed changes $\Delta \log \left(1 - \tau_s^j \right)$. Analogously, we simulate post-reform virtual incomes under pre-reform behavior, $y_{s+3} \left(z_s^1, \dots, z_s^n \right) = \sum_{j=1}^N \tau_{s+3}^j \left(z_s^1, \dots, z_s^n \right) z_s^j - T_{s+3} \left(z_s^1, \dots, z_s^n \right)$, and associated mechanical changes in virtual income, $\log \left(y_{s+3} \left(z_s^1, \dots, z_s^n \right) \right) - \log \left(y_s \left(z_s^1, \dots, z_s^n \right) \right)$, which are used as instruments for the observed changes $\Delta \log \left(y_s \right)$.

While the mechanical tax changes used as instruments are exogenous to post-reform incomes, they do depend on pre-reform incomes. Hence, the instruments may be correlated with the error term if the pre-reform income level is correlated with the error term. The literature has discussed two channels through which this may occur. First, tax payers at different pre-reform income levels may experience different income trends for non-tax reasons. Indeed, many countries have experienced sharply increasing top income shares over the past few decades, and several studies have argued that these changes are driven by skill-biased demand shocks resulting from innovation and globalization. Unless skill can be directly controlled for, it would be captured by pre-reform income levels and skill-biased changes would then be absorbed in the estimated elasticity. Second, the pre-reform income level reflects both permanent and transitory income components, which creates a mean-reversion problem: a tax payer with a very high income in the pre-reform year will tend to have a lower income in the post-reform year, other things being

equal. In the absence of controls for transitory income components, they would be captured by pre-reform income levels and hence be absorbed by the estimated tax effect.

The problems just described are particularly acute when considering tax reforms that are strongly targeted to certain income groups such as high-income earners (as in the case of the U.S. tax reforms in the 1980s). In that case, the mechanical tax changes will be strongly correlated with income level and therefore with skill-dependent demand shocks and transitory income components. To deal with this problem, Auten and Carroll (1999), Moffitt and Wilhelm (2000), Gruber and Saez (2002), and Kopczuk (2005) propose to control in different ways for pre-reform income. For example, Kopczuk (2005) proposes a specification that includes the change in income in the year prior to the reform, $z_s - z_{s-1}$, as a proxy for transitory income components, along with the lagged income level z_{s-1} as a proxy for the permanent income level. He allows for nonlinearity by experimenting with 10-piece splines in the logarithms of either of the two controls. He also explores a number of other specifications, including those adopted by Auten and Carroll (1999) and Gruber and Saez (2002). The results show that the elasticity estimates are extremely sensitive to the specification of pre-reform income controls.

We consider the various income controls that have been proposed in the earlier studies, along with specifications that control in a richer way for the dynamic income process. The rich tax variation in Denmark allows us to include additional lags and higher-order splines without soaking up all the identifying variation. Unlike the previous literature, we find that our results are extremely robust to the specification of income controls, suggesting that we have controlled in a sufficiently rich way for non-tax factors impacting on taxable income.

There are three key reasons for the robustness of our findings. First, because of the large set of socioeconomic variables in the data, we are able to control directly for a number of non-tax characteristics driving permanent and transitory income components. For permanent income, variables on education (level attained and area of study) capture skill level, which controls for the effect of a changing income distribution due to skill-biased demand shocks. Variables on gender, marriage, kids, immigration, and ethnicity may further control for permanent income components. For transitory income, controlling for variables such as place of residence, local unemployment, age, job experience, occupation, and industry ensure that we are comparing individuals affected similarly by idiosyncratic labor market conditions, which alleviates the problem of mean-reversion. Second, as discussed in the introduction and shown in Figure 1, there has

been no significant secular change in the income distribution in Denmark, implying that the bias from unobserved non-tax factors affecting the income distribution is not a big concern here. This isolates mean-reversion as the potential bias that the income controls have to correct for (to the extent that the other socioeconomic variables are not sufficient). Third and crucially, the biases discussed above rely on the presence of a correlation between tax changes and pre-reform income level, which is not an important feature of the Danish reforms. As described earlier, the Danish reforms were not systematically targeted to certain income groups and created a lot of up-and-down movements in tax rates throughout the income distribution. In fact, the increasing asymmetry in the tax treatment of different income components creates variation even for taxpayers at the same income level (but with different income compositions). In the next section, we demonstrate the exact nature of the Danish variation around specific reform episodes.

4.3 Mechanical Variation in Marginal Net-of-Tax Rates

To give a precise sense of the identifying variation, Figures 3-5 show the mechanical variation in marginal net-of-tax rates (i.e., the variation in the instrument) for different income types around the two largest reform episodes in our data, the 1987- and 1994-reforms. Each figure shows three-year differences in percent, where we have split the sample into seven groups using base-year income variables: (*i*) individuals who are in the bottom bracket both before and after, (*ii*) individuals who are pushed from the middle to the bottom bracket, (*iii*) individuals who are pushed from the bottom to the middle bracket, (*iv*) individuals who are in the middle bracket both before and after, (*v*) individuals who are pushed from the top to the middle bracket, (*vi*) individuals who are pushed from the middle to the top bracket, and (*vii*) individuals who are in the top bracket before and after. It is important that the grouping is based only on base-year income variables and therefore does not incorporate a behavioral response.

Before considering the tax variation in Figures 3-5, let us briefly remark on two aspects of the figures. First, it is the combination of changes in tax bases and bracket cutoffs that makes it possible for a tax reform to push some taxpayers from a lower to a higher bracket (e.g. bottom to middle) and simultaneously push other taxpayers in the opposite direction (e.g. middle to bottom). Second, the grouping of taxpayers in the figure is useful to make the identifying tax changes stand out. The grouping is different from one based on quantiles of the income

distribution (for e.g. total taxable income). Such a grouping would show much less *average* tax variation in each quantile group as it lumps together tax reductions for those who stay in a given bracket or move to a lower bracket with tax increases for those who are pushed into a higher bracket. Hence, an income quantile representation of tax changes would hide a lot of the identifying variation in the data.

Each figure shows the sizes of the different groups as a share of all taxpayers (red bars) along with the mechanical changes in the marginal net-of-tax rate in the different groups (blue graph).⁸ Figure 3 illustrates the variation in labor income taxation around the 1987-reform (1986-1989 difference) in Panel A and around the 1994-reform (1993-1996 difference) in Panel B. For the 1987-reform, there are very large *and* strongly heterogeneous tax changes across taxpayers, with the percentage change in the net-of-tax rate varying between -20% and +42%. These differences in tax treatments across groups are larger than the tax treatment differences created by the Tax Reform Act of 1986 in the U.S. and the Tax Reform of the Century in Sweden in 1991, two reforms that have been extensively analyzed in the literature. For the 1994-reform, tax changes are also very large and heterogeneous, but not quite to the same degree as for the 1987-reform.

Figures 4 and 5 show the variation in the taxation of negative and positive capital income around the same two reform episodes. The figures are constructed in the same way as Figure 3 above. For the 1987-reform, the tax variation on capital income, especially negative capital income, is even stronger than for labor income. The marginal net-of-tax rate for those in the top bracket increased by more than 50% (40%) in the case of negative (positive) capital income, while other groups of taxpayers experienced much smaller increases or reductions in the net-of-tax rate. The 1994-reform have much smaller effects than the 1987-reform and, importantly, the tax variation created by the 1994-reform is qualitatively very different. For positive capital income, for example, the net-of-tax rate is reduced at the top and increased at the bottom directly opposite the 1987-reform.

Although the tax changes around 1987 and 1994 constitute the strongest variation in the data, there is in fact a lot of variation throughout the period we consider (1984-2005). Importantly, the tax variation in other years is often qualitatively different in terms of who experience tax increases and who experiences tax cuts.

⁸The population shares of the seven groups do not quite sum to 100% due to a small number of taxpayers below the exemption level for the bottom bracket.

5 Empirical Results

In the following, we describe the results from 2SLS-estimations using mechanical tax changes as instruments. We present separate estimations for labor income, negative capital income, and positive capital income. The first-stage regressions (not shown) are always very strong. For the second stage, we focus on the key elasticity parameters of interest and do not show point estimates associated with the large number of covariates that we control for. The exact details of the different regression specifications that we consider are provided in the notes of regression tables. In all regressions, standard errors are clustered by individual in order to account for any individual-specific correlation in income changes over time.

Labor Income Elasticities

The first set of results is presented in Table 4, which shows estimates of labor income elasticities based on specifications that assume no income effects and no cross-tax effects. The potential importance of income effects and cross-tax effects are considered later on. The table splits the sample by wage earners (Panel A) and self-employed individuals (Panel B), and shows results for a number of different specifications and sample restrictions that have been discussed in the literature.

The different rows in the table consider different specifications of pre-reform income controls: no income controls, the different income controls proposed by Gruber and Saez (2002) and Kopczuk (2005), along with specifications that include richer controls for transitory income components by including log-income changes from period $s - 2$ to $s - 1$ and from $s - 1$ to s . Results in the previous literature have been extremely sensitive to the specification of pre-reform income controls. The different columns in the table consider different sample restrictions (in addition to the basic restrictions described in section 3): all taxpayers with positive broad income (defined as the sum of labor income, other personal income and capital income) in columns (1) and (5), taxpayers with broad income above DKK 50,000 (about USD 10,000) in columns (2) and (6), and taxpayers with broad income above DKK 100,000 (about USD 20,000) in columns (3) and (7). Results in the previous literature have been very sensitive to such income restrictions due to mean reversion at the bottom. Finally, columns (4) and (8) consider results when taxpayers located close to kink points are excluded. This is done because the Gruber-Saez style specifications we consider assume that taxpayers behave as if they are located in the

interior of brackets and do not bunch at kink points. If there is significant bunching at kink points, this may create bias in the estimates. As shown by Chetty et al. (2011) and Kleven et al. (2010), there is indeed bunching at the top kink in Denmark (but not at the bottom and middle kinks) and we therefore investigate if our results are sensitive to this.

The table shows that results are extremely robust to both the specification of pre-reform income controls and sample selection. While it does matter whether *any* income controls are included (first row versus subsequent rows), the exact specification of pre-reform income controls is not important. Moreover, excluding taxpayers at the bottom (to avoid mean reversion at the bottom) and excluding taxpayers around kink points (to avoid results being attenuated by bunching) have very little impact on the results.⁹ Across all the specifications shown in the table (ignoring the first row without any income controls), the elasticity of labor income vary between 0.04 and 0.06 for wage earners and between 0.08 and 0.10 for self-employed individuals.

In the following tables, we no longer show the different sample restrictions considered in Table 4, because those sample restrictions do not matter for any of our results. In all the tables below, we include all taxpayers with positive broad income and do not drop observations around kink points (as in columns (1) and (5) of Table 4). Table 5 consider the importance of income effects by including virtual income in the specification, and again we split the sample by wage earners and self-employed individuals. As a benchmark, columns (1) and (4) repeat results from a specification without income effect as in Table 4. Those columns are labelled “compensated elasticity”, although in a specification without income effects there is of course no distinction between the compensated and uncompensated elasticity. Columns (2)-(3) and (5)-(6) show the uncompensated elasticity and income elasticity based on specifications with income effects. In those specifications, the compensated elasticity is approximately equal to the uncompensated elasticity minus the income elasticity.¹⁰

The general finding in Table 5 is that income elasticities are negative, implying that leisure

⁹It is not very surprising that bunching around kink points has no significant effect on our results. Although there is clear bunching at the top kink in Denmark, it affects a small part of the population (see Table 3) and is small in magnitude, especially for wage earners where the elasticity implied by bunching is only 0.01 (Chetty et al. 2011). Bunching is stronger for self-employed individuals, which is consistent with our finding that the impact of excluding taxpayers around kink points is slightly larger for the self-employed than for wage earners. Notice also that, as one would expect, elasticities become larger when excluding observations close to kinks, because those taxpayers are constrained in their response to the reform-driven tax variation that we use for identification.

¹⁰This is an approximation because the income elasticity should be weighted by labor income as a share of total income. Since this share is close to one for most taxpayers, this weighting have no large impact on the calculation of the compensated elasticity.

is a normal good, but very small. The finding of small income effects is consonant with the public finance and labor economics literatures in general. While the point estimates of income effects are roughly the same for wage earners and the self-employed, the income effects are statistically significant only for wage earners where we have many more observations than for the self-employed. In all specifications, the compensated elasticity implied by the specification with income effects correspond closely to the elasticity obtained from the specification without income effects. Importantly, the results in Table 5 are again extremely robust to the specification of pre-reform income controls.

Capital Income Elasticities

We now turn to the analysis of capital income responses. Capital income is a net income concept that may be either positive or negative, and is in fact negative for the vast majority of taxpayers in Denmark due to interest payments on mortgages and other loans. As described in section 2, the tax treatment of capital income is very different depending on whether the net value is positive or negative, with much higher tax rates on positive than on negative capital income. Since we consider log-linear regression specifications that do not allow for non-positive income values, we consider capital income in absolute value and run separate regressions for negative and positive capital income.¹¹

The results are shown in Table 6, which compares elasticities for negative and positive capital income in columns (2) and (3) to elasticities for labor income in column (1). The table shows results for all taxpayers (wage earners and self-employed individuals together) and is based on specification without income effects. Notice that we would expect the elasticity of negative capital income (in absolute value) to be negative and the elasticity of positive capital income to be positive, and this is indeed what we find for all specifications. Overall, capital income elasticities tend to be 2-3 times larger in absolute value than labor income elasticities, and

¹¹This strategy requires us to drop individuals with zero capital income as well as those whose capital income switch sign between base and post year. An additional argument for dropping observations around zero capital income is that the imposition of much higher marginal tax rates on positive than on negative capital income (after the 1987-reform) creates a large kink in the capital income tax schedule at zero. This is associated with strong incentives for bunching at zero capital income, a type of response that is not captured by the Gruber-Saez estimation strategy and may create bias as discussed earlier. Indeed, we find strong bunching in the data around zero capital income. While this is interesting by itself and might offer a different way of uncovering capital income elasticities, a key problem of exploiting bunching at zero capital income is that it is likely to partly reflect non-tax factors. Even without the tax kink, there would have been some excess clustering at zero as many taxpayers have not accumulated any saving or debt because of their stage in the life cycle (e.g. young taxpayers) or because of credit constraints.

again the results are very robust the specification of pre-reform income controls. Elasticities of negative capital income vary between -0.07 and -0.13 across all specifications, while elasticities of positive capital income vary between 0.08 and 0.14.

Responses to Small vs. Large Tax Reforms

In general, the responses estimated above are fairly modest, consistent with many other micro studies of intensive responses to taxation such as hours-of-work responses (e.g. Heckman 1993; Blundell and MaCurdy 1999; Meghir and Phillips 2010; Chetty 2011). An important question is whether micro elasticities are small because they are attenuated by optimization frictions (such as inattention and adjustment costs) or because the “true” structural elasticity that overcomes optimization frictions and matters for long-run behavior is small. As argued by Chetty et al. (2011) and Chetty (2011), the estimation of long-run structural elasticities requires tax variation that is large enough to overcome frictions. The Danish setting allows us to explore this question, because the time period we consider includes one very large tax reform episode along with many smaller tax reform episodes. In particular, the 1987-reform was associated with extremely large tax variation (as shown in section 4.3) and this reform is perhaps the most widely discussed and salient tax reform in Danish history. To test the hypothesis that elasticities are larger when estimated from large tax changes as they overcome frictions and reveal the long-run structural elasticity, we compare responses to the 1987-reform with responses to all the subsequent smaller changes.

The results are shown in Table 7 for all reforms (1984-2005 period), 1987-reform (1984-1990 period), and post-1987 reforms (1991-2005 period) for labor income (Panel A) and positive capital income (Panel B). The elasticity estimations include all taxpayers (wage earners and self-employed individuals together) and are based on specifications without income effects. We do not show results for negative capital income, because it is associated with very little identifying variation after the 1987-reform as there was just one bracket for negative capital income through most of this period (see Figure 2, Panel B). The lack of variation for negative capital income after the 1987-reform makes it difficult to separately estimate responses for this period and income type in a robust way.

The results in the table lend clear support to the hypothesis that micro elasticities are larger when estimated using large tax variation. The labor income elasticity estimated from the 1987-

reform is about 0.11-0.12, which is 2-3 times larger than elasticities based on the whole period and 3-6 times larger than elasticities based on the post-1987 reforms alone. These results are again robust to pre-reform income controls. Results for capital income are qualitatively similar, but the difference between large-reform elasticities (0.14-0.15) and small-reform elasticities (0.08-0.10) is not as strong as for labor income. It is intuitive that the size of the tax change matters more for labor income than for capital income, because labor income responses are likely to be affected by real adjustment costs (e.g. search costs) to a larger extent than capital income responses. On the other hand, frictions due to for example inattention would matter for both labor and capital income.

The results in Table 7 are qualitatively consistent with the findings of Chetty et al. (2011), who compare bunching around small and large kinks in the Danish tax code to evaluate the importance of frictions in attenuating short-run responses and to estimate the long-run elasticity that overcomes frictions. But the magnitude of the elasticity that we find using the large 1987-reform is much larger than the elasticity found by Chetty et al. (2011) using bunching at the large top-tax kink. A likely explanation is that bunching around kink points, even large kink points, are much more affected by frictions due to the fact that bunching requires precise knowledge of bracket thresholds along with a very precise behavioral response, both of which may be costly to achieve due to adjustment costs, attention costs, etc. The reform-driven variation we consider is likely to be less affected by such aspects.

Cross-Tax Effects Between Labor and Capital Income:

The specifications so far have ignored potential cross-tax effects between labor and capital income. Table 8 considers such effects by presenting results from labor income regressions that include the marginal net-of-tax rates on both labor income and capital income. The specification does not include income effects on labor income, which are in any case weak and have little impact on the results. The table considers all taxpayers (Panel A), wage earners (Panel B), and self-employed individuals (Panel C). Each panel shows the own-tax elasticity on the left and the cross-tax elasticity on the right. As in all other tables, we consider a number of different specifications of pre-reform income controls.

Overall, the table suggests that cross-tax effects are not very important in the Danish tax system. Cross-tax elasticities are in general very small (and sometimes insignificant), and al-

lowing for such cross-tax effects have very little impact on the own-tax elasticity compared to the earlier tables that ignores those effects. For wage earners, the cross-tax elasticity is positive and very small, suggesting weak complementarity between labor and capital for those individuals. For the self-employed, the cross-tax elasticity is negative and somewhat larger in absolute value (but not precisely estimated), suggesting weak substitutability between labor and capital income for those individuals. The finding that labor and capital income are substitutes for the self-employed is consistent with income shifting in response to differential tax treatment of the two income forms. While income shifting has been much discussed in the public finance literature (e.g. Slemrod 1995, 1998; Saez, Slemrod, and Giertz 2010), there exists relatively little direct evidence on this type of behavioral response.¹²

6 Conclusion

This paper presents an empirical study of taxable income responses using a series of tax reforms that provide substantial tax variation across individuals, income forms, and over time. The variation provided by the Danish tax reforms does not feature the same strong correlation between tax changes and income levels as the tax reforms in the U.S. (and other countries) that have been extensively studied in the past two decades. Arguably, this allows us to overcome the identification problems arising from non-tax changes in the income distribution and mean-reversion that plague much of the existing literature (as discussed by e.g. Saez, Slemrod, and Giertz 2010). Unlike previous studies, our results are very robust to the specification of controls for non-tax changes in the income distribution and mean reversion, which suggests that we have controlled in a sufficiently rich way for non-tax factors impacting on taxable income. The Danish setting therefore offers a useful laboratory for a credible estimation of taxable income responses.

As pointed out by Slemrod (1998) and Slemrod and Kopczuk (2002), taxable income elasticities are not structural parameters that depend only on individual preferences. They depend in important ways on the opportunities for tax avoidance and tax evasion, which in turn depend on the tax structure (especially the broadness of tax bases) and on tax enforcement. The fairly low taxable income elasticities that we find for Denmark, despite the presence of very high marginal

¹²Previous evidence on income shifting include Pirttilä and Selin (2011) on shifting between labor and capital income in Finland, Slemrod (1996) and Gordon and Slemrod (2000) on shifting between personal and corporate income in the United States, and Kleven and Waseem (2011) on shifting between wage income and self-employment income in Pakistan.

tax rates, suggests that the Danish system offers small opportunities for avoidance and evasion. There are two main reasons for this. First, tax bases are very broad and offer limited opportunities for deductions and negative capital income to count against the income tax base. Second, as shown by Kleven et al. (2011), tax enforcement is very effective and overall tax compliance is high due to the widespread use of double-reporting by third parties such as employers and financial institutions. The overall conclusion that emerges from the two studies together is that a tax system with the broadest possible bases and extensive use of information reporting can impose high marginal tax rates with fairly modest behavioral responses.

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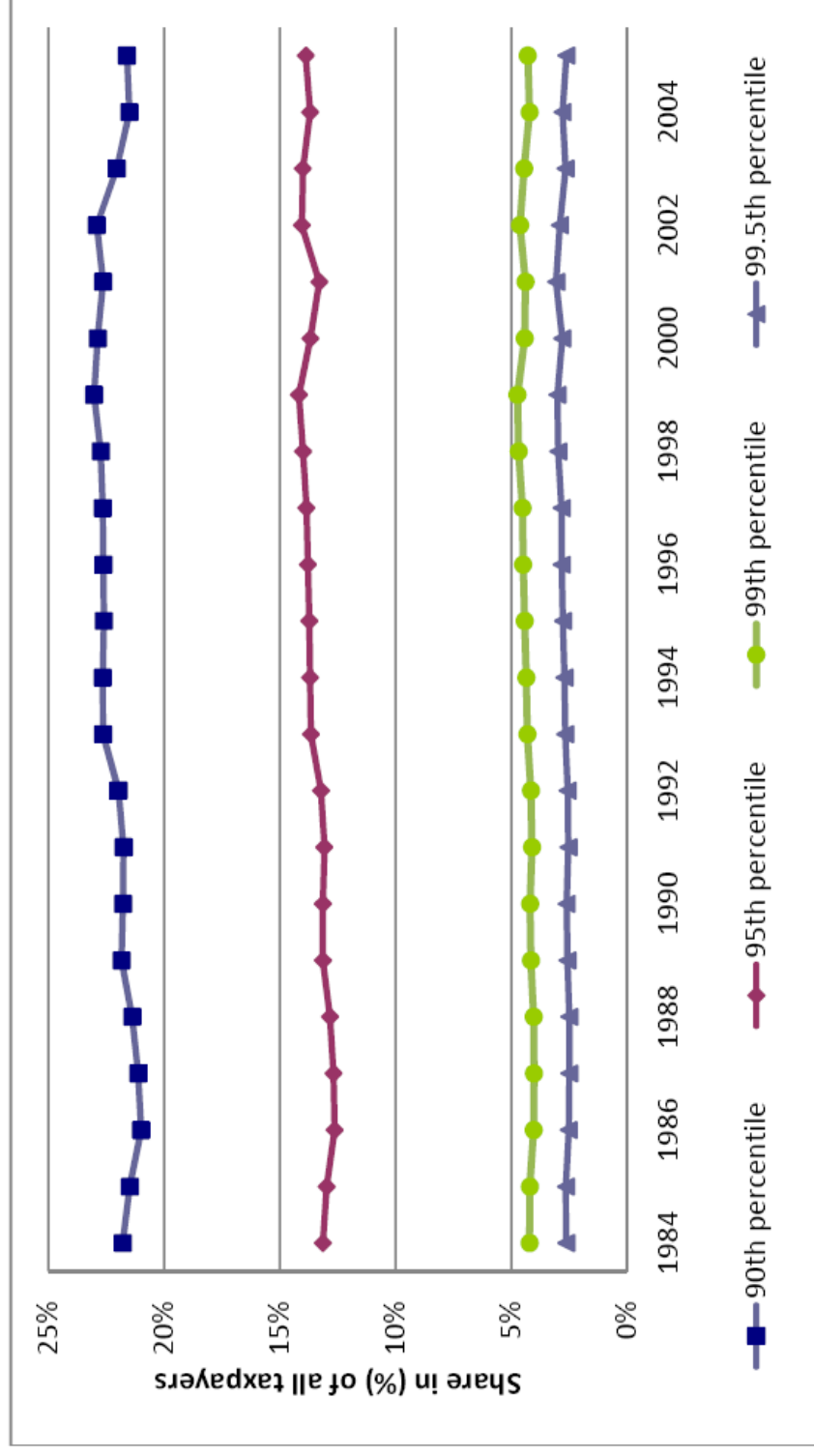
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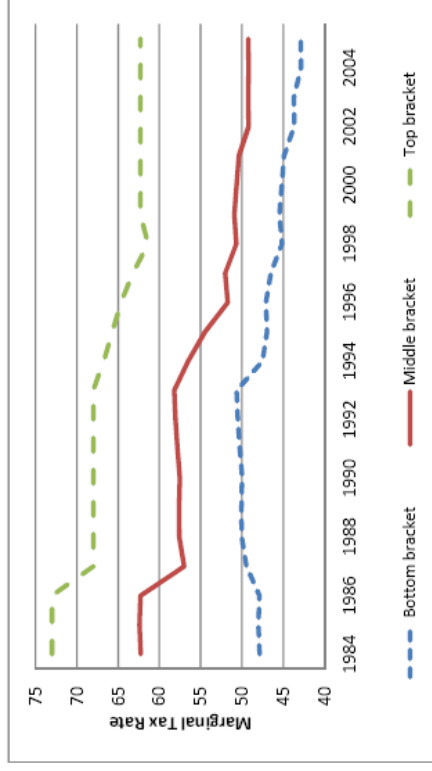
Figure 1. The Evolution of Top Income Shares in Denmark



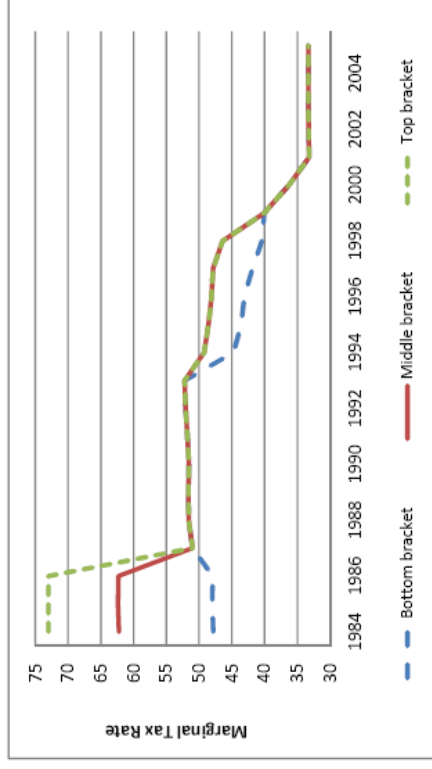
Notes: The income shares are based on income tax return information and consider a broad income measure that includes labor income, other personal income and capital income (see Table 1 for details about these income components). The sample includes all personal income tax filers aged 25 to 55.

Figure 2. Two Decades of Danish Tax Reform

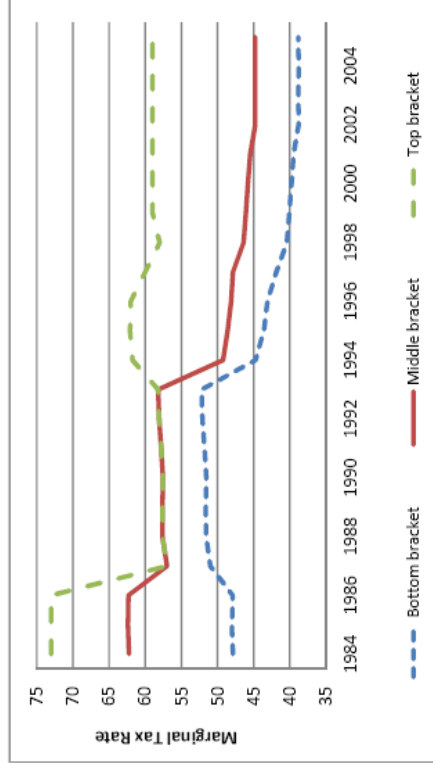
Panel A. Marginal Tax Rate on Labor Income



Panel B. Marginal Tax Rate on Negative Capital Income



Panel C. Marginal Tax Rate on Positive Capital Income



Panel D. Share of Taxpayers in the Three Tax Brackets

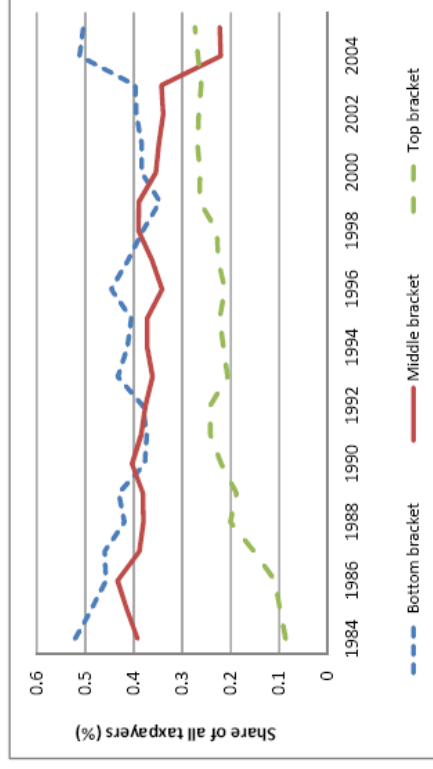
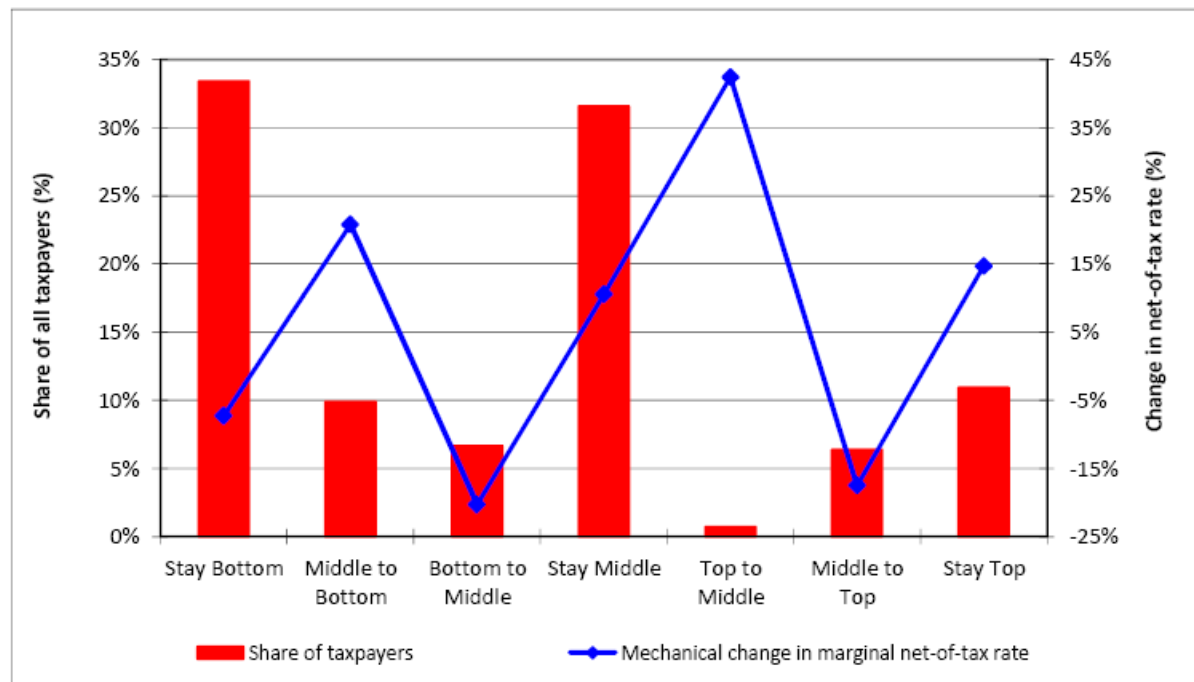


Figure 3. Mechanical Variation in the Marginal Net-of-Tax Rate on Labor Income

Panel A. 1987-Reform (1986-1989 Difference)



Panel B. 1994-Reform (1993-1996 Difference)

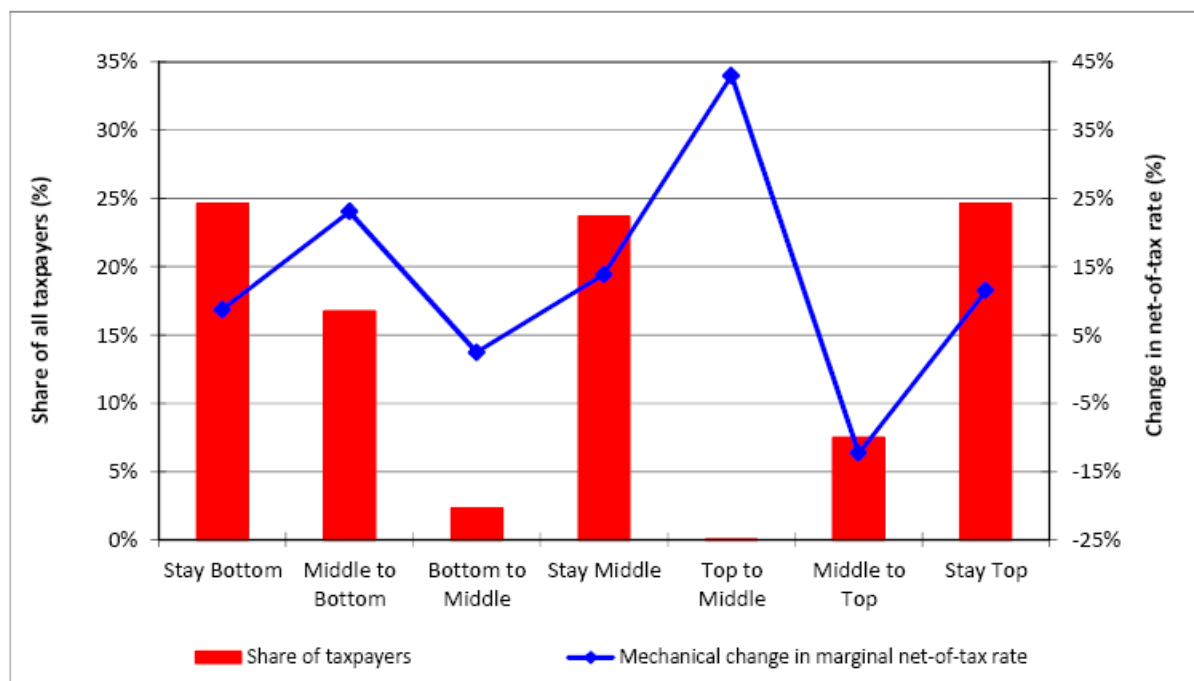
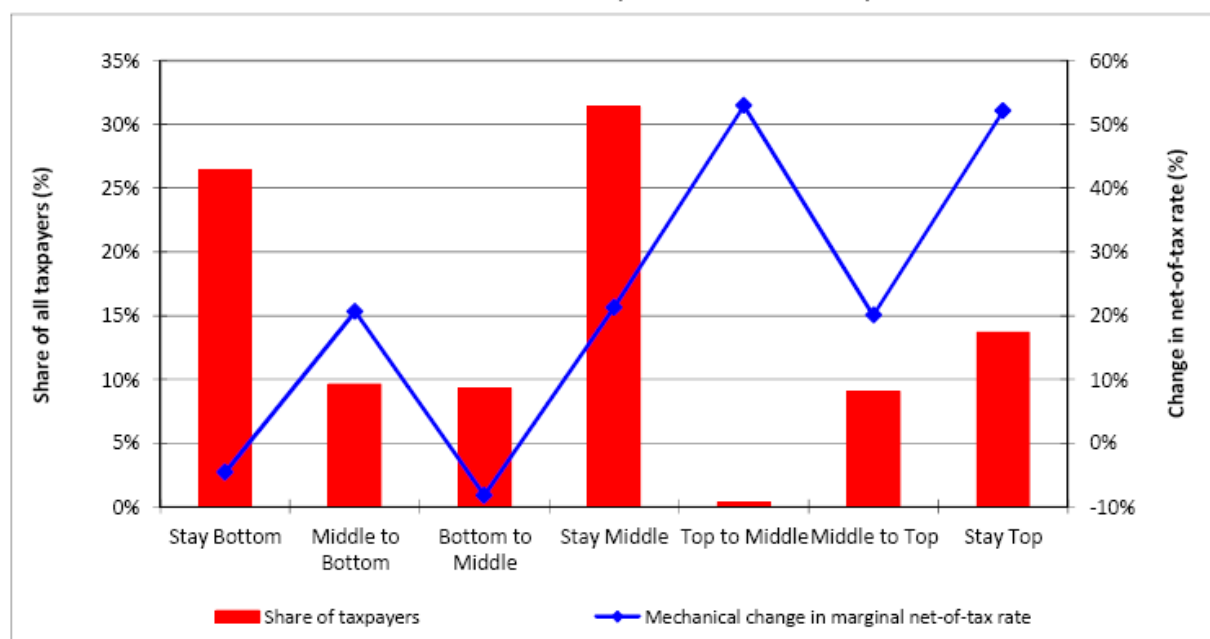


Figure 4. Mechanical Variation in the Marginal Net-of-Tax Rate on Negative Capital Income

Panel A. 1987-Reform (1986-1989 Difference)



Panel B. 1994-Reform (1993-1996 Difference)

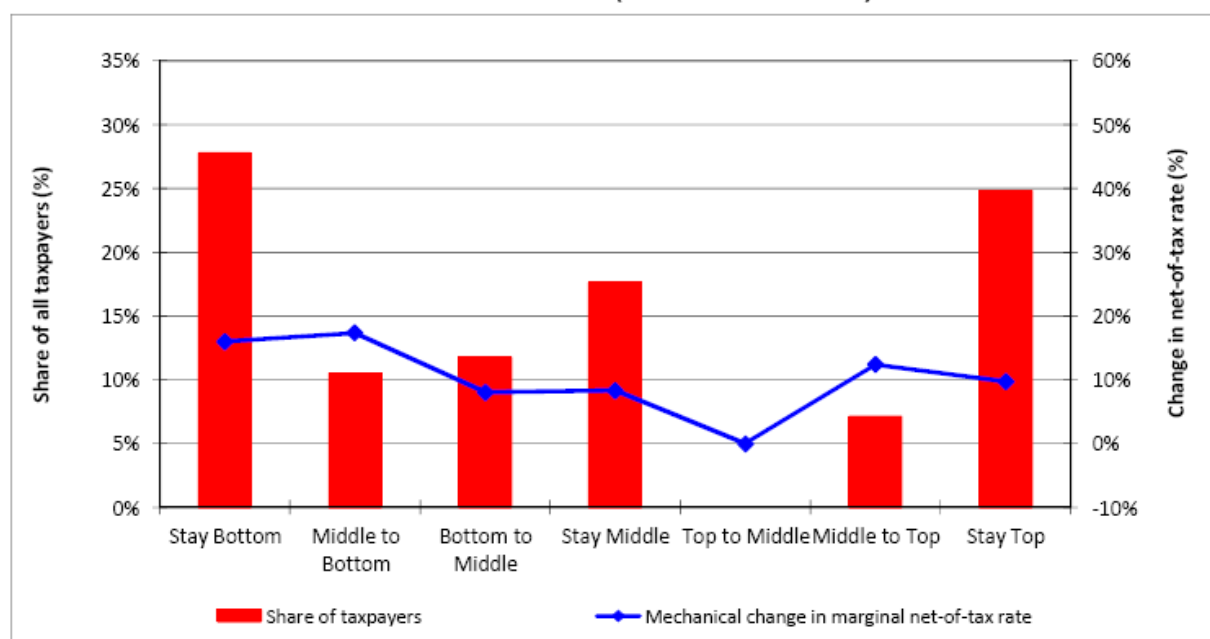
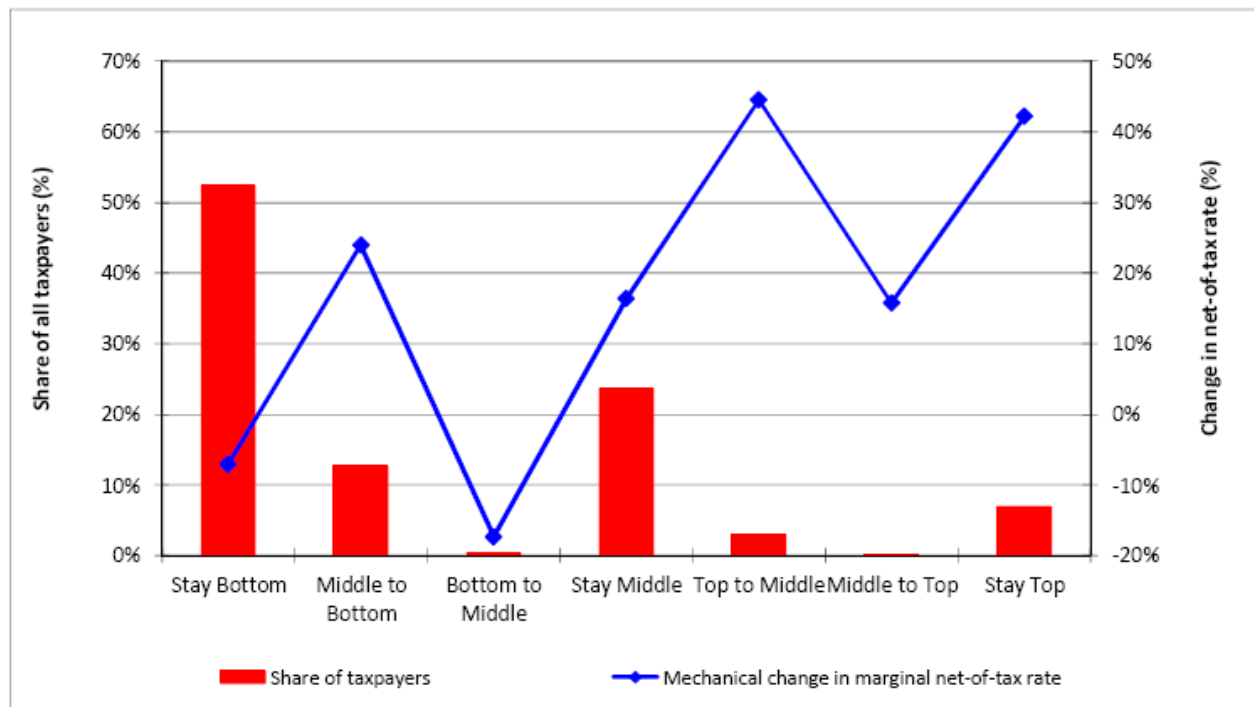


Figure 5. Mechanical Variation in the Marginal Net-of-Tax Rate on Positive Capital Income

Panel A. 1987-Reform (1986-1989 Difference)



Panel B. 1994-Reform (1993-1996 Difference)

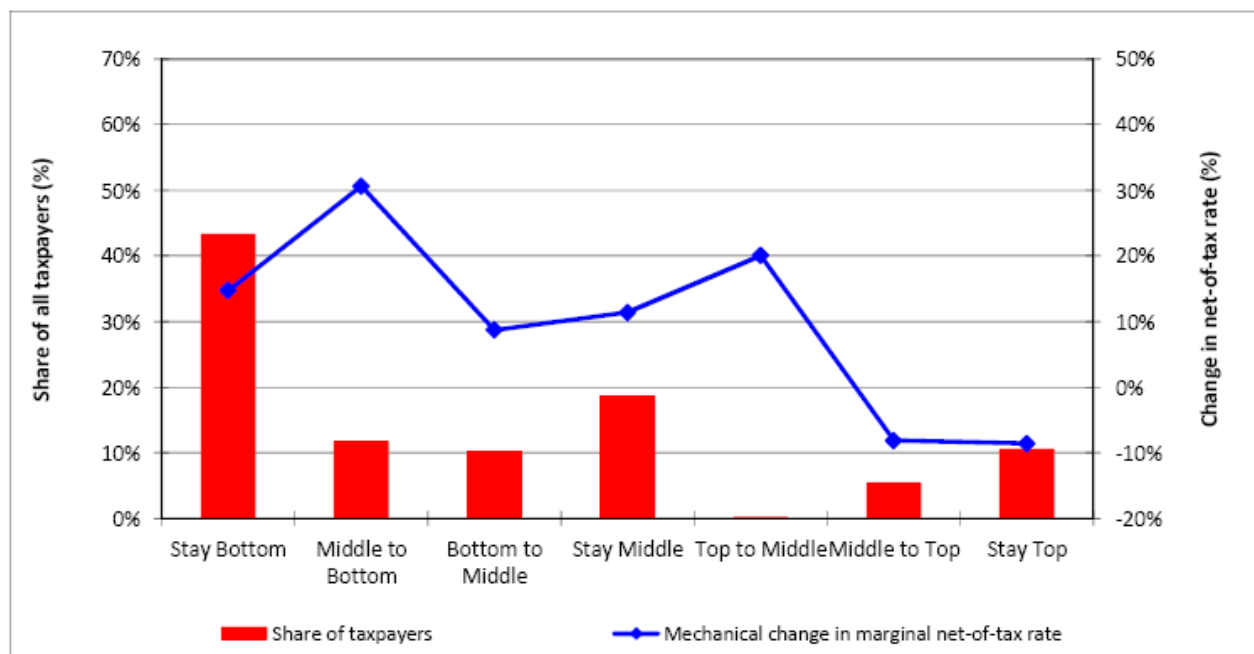


Table 1. Income Concepts in the Danish Individual Income Tax

Income concept	Acronym	Main items included
1. Labor Income	LI	Salary, wages, honoraria, fees, bonuses, fringe benefits, business earnings
2. Personal Income	PI	LI + transfers, grants, awards, gifts, received alimony – Labor Market Contribution, certain pension contributions
3. Capital Income	CI	Interest income, rental income, business capital income – interest on debt (mortgage, bank loans, credit cards, student loans)
4. Stock Income	SI	Dividends and realized capital gains from shares
5. Deductions	D	Commuting, union fees, UI contributions, other work expenditures, charity, paid alimony
6. Taxable Income ¹	TI	PI + CI + SI – D

1. The definition of taxable income in this table does not correspond to what is currently labelled “taxable income” in the Danish tax code, which excludes stock income as it is taxed on a separate schedule (see Table 2 below).

Table 2. Tax Bases and Tax Rates over Time in the Danish Individual Income Tax System

Tax type ¹	1986		1993		1998		2005	
	Base	Rate (%)	Base	Rate (%)	Base	Rate (%)	Base	Rate (%)
Regional tax ²	TI	28.1	PI + CI - D	30.2	PI + CI - D	32.4	PI + CI - D	33.3
National taxes:								
Bottom tax	TI	14.4	PI + CI - D	22.0	PI + CI - D	8.0	PI + [CI > 0]	5.5
Middle tax	TI	14.4	PI + [CI > 0]	6.0	PI + CI	6.0	PI + [CI > 0]	6.0
Top tax	TI	10.8	PI	12.0	PI + [CI > 21k]	15.0	PI + [CI > 0]	15.0
Social security contribution	TI	5.5	-	-	-	-	-	-
Labor market contribution ³	-	-	-	-	LI	8.0	LI	8.0
EITC	-	-	-	-	-	-	LI	2.5
Tax on stock income ⁴	-	-	SI	30.0; 40.0	SI	25.0; 40.0	SI	28.0; 43.0
Marginal tax ceiling ⁵	TI	73.0	PI/CI/TI	68.0	PI/CI/TI	58.0	PI/CI/TI	59.0

1. Tax rates are cumulative. For example, the marginal tax rate in the top bracket in 1986 is equal to $28.1 + 14.4 + 10.8 + 5.5 = 73.2\%$ (but see footnote 4 regarding marginal tax ceiling adjustment)

2. The regional tax includes municipal, county, and church taxes. The regional tax rate in the table is an average across all municipalities in Denmark in each year.

3. After the introduction of the labor market contribution, labor income enters the other tax bases net of the labor market contribution. Hence, in those years, the effective tax rate on labor income equals the statutory tax rate times $(1 - \text{labor market contribution})$.

4. After the 1987-reform, the taxation of stock income is completely separate from the rest of the income tax and follows a two-bracket progressive schedule with the marginal tax rates shown in the table.

5. If the sum of all regional and national tax rates (excluding the stock income tax after the 1987-reform) exceeds the specified ceiling, the top tax is adjusted downwards until the the marginal tax rate equals the ceiling.

Table 3. Summary Statistics for the Estimation Sample, 1984-2005

	Full Sample	Wage Earners	Self-Employed
	(1)	(2)	(3)
Demographics:			
Age	40.5	40.0	48.3
Number of children (0-17 years)	0.7	0.7	0.7
Labor market experience (years)	13.2	13.6	7.2
Male	52.5%	51.3%	72.2%
Married	55.1%	54.1%	71.4%
Primary and secondary education	41.8%	41.8%	42.7%
Vocational education	41.8%	41.6%	44.1%
Tertiary education	16.4%	16.6%	13.2%
Taxable Income:			
Labor Income	247,935	249,328	226,275
Other personal income	3,204	737	41,554
Capital income	-27,585	-27,760	-24,853
Deductions	16,056	16,490	9,299
Share of Taxpayers Close to Kink Points:			
Top kink	2.39%	2.01%	8.37%
Middle kink	2.36%	2.40%	1.74%
Bottom kink	1.20%	1.04%	3.78%
Number of observations	37,599,492	35,326,867	2,272,625

Notes: Table entries are means unless otherwise noted. All monetary values are in real 2005 Danish Kroner (DKK), where 1 USD = 5.3 DKK as of July 2011. Taxpayers close to kink points are defined as those who have an income within a range of 5,000 DKK of the top kink, 3,000 DKK of the middle kink and 2,000 DKK of the bottom kink.

Table 4. The Elasticity of Labor Income

	Panel A. Wage Earners				Panel B. Self-Employed			
	Taxpayers around kinks:							
	Include	Exclude	Include	Exclude	Include	Exclude	Include	Exclude
Broad income restriction:	> 0k	> 0k	> 50k	> 100k	> 0k	> 50k	> 100k	> 0k
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Pre-reform income controls								
No pre-reform income controls	-0.191*** (0.002)	-0.123*** (0.002)	-0.102*** (0.002)	-0.132*** (0.002)	-0.155*** (0.013)	-0.141*** (0.014)	-0.126*** (0.015)	-0.162*** (0.015)
Log base-year (period s) income	0.060*** (0.002)	0.067*** (0.002)	0.065*** (0.002)	0.062*** (0.002)	0.095*** (0.015)	0.088*** (0.015)	0.085*** (0.016)	0.098*** (0.016)
Splines of log base-year (period s) income	0.042*** (0.002)	0.042*** (0.002)	0.044*** (0.002)	0.043*** (0.002)	0.100*** (0.013)	0.096*** (0.014)	0.093*** (0.014)	0.101*** (0.015)
Splines of log s-1 income and log deviation between s-1 and s incomes	0.046*** (0.002)	0.051*** (0.002)	0.052*** (0.002)	0.047*** (0.002)	0.090*** (0.014)	0.083*** (0.015)	0.081*** (0.015)	0.094*** (0.016)
Splines of log s-1 income and splines of log deviation between s-1 and s incomes	0.042*** (0.002)	0.046*** (0.002)	0.047*** (0.002)	0.045*** (0.002)	0.087*** (0.014)	0.081*** (0.015)	0.078*** (0.015)	0.093*** (0.017)
Splines of log s-2 income and log deviations between s-2 and s-1 incomes and between s-1 and s incomes	0.056*** (0.002)	0.062*** (0.002)	0.061*** (0.002)	0.055*** (0.002)	0.098*** (0.015)	0.093*** (0.015)	0.090*** (0.015)	0.101*** (0.017)
Number of observations	29,568,870	28,630,140	27,121,055	28,060,857	1,646,270	1,568,195	1,381,560	1,405,915

Notes: The table shows elasticity estimates based on 2SLS regressions, where standard errors (shown in parentheses) are clustered by individual. * = significant at the 10% level, ** = significant at the 5% level, and *** = significant at the 1% level. The dependent variable in all specifications is the three-year growth rate in real wage earnings. The independent variable of interest is the three-year growth rate in the marginal net-of-tax rate, instrumented using the three-year growth rate in the simulated marginal net-of-tax rate under base-year behavior (i.e., mechanical tax variation from tax reforms). All elasticities in the table are based on specifications without income effects. All specifications include controls for labor market experience, age, gender, marital status, kids, place of residence, educational attainment, industry, local unemployment rate, and year fixed effects. Regressions are weighted by labor income and restricts the sample to individuals with positive labor income (in addition to the sample restrictions described in section 3). "Splines" refer to a flexible piecewise linear functional form with 10 components. Taxpayers close to kink points are defined as those who have an income within a range of 5,000 DKK of the top kink, 3,000 DKK of the middle kink and 2,000 DKK of the bottom kink.

Table 5. The Elasticity of Labor Income: Compensated vs. Uncompensated Elasticity

	Panel A. Wage Earners			Panel B. Self-Employed		
	Without income effect		With income effect	Without income effect		With income effect
	Compensated elasticity (1)	Uncompensated elasticity (2)	Income Elasticity (3)	Compensated elasticity (4)	Uncompensated elasticity (5)	Income Elasticity (6)
Pre-reform income controls						
No pre-reform income controls	-0.191*** (0.002)	-0.140*** (0.003)	-0.007*** (0.002)	-0.155*** (0.013)	-0.159*** (0.030)	-0.003 (0.021)
Log base-year (period s) income	0.060*** (0.002)	0.042*** (0.003)	-0.015*** (0.003)	0.095*** (0.015)	0.114*** (0.033)	-0.015 (0.021)
Splines of log base-year (period s) income	0.042*** (0.002)	0.034*** (0.003)	-0.007*** (0.003)	0.100*** (0.013)	0.105*** (0.032)	-0.004 (0.021)
Splines of log s-1 income and log deviation between s-1 and s incomes	0.046*** (0.002)	0.030*** (0.003)	-0.013*** (0.002)	0.090*** (0.014)	0.096*** (0.032)	-0.005 (0.021)
Splines of log s-1 income and splines of log deviation between s-1 and s incomes	0.042*** (0.002)	0.030*** (0.003)	-0.011*** (0.002)	0.087*** (0.014)	0.093*** (0.032)	-0.004 (0.021)
Splines of log s-2 income and log deviations between s-2 and s-1 incomes and between s-1 and s incomes	0.056*** (0.002)	0.034*** (0.003)	-0.019*** (0.002)	0.098*** (0.015)	0.096*** (0.030)	-0.001 (0.020)
Number of observations		29,568,870			1,646,270	

Notes: The table shows elasticity estimates based on 2SLS regressions, where standard errors (shown in parentheses) are clustered by individual. * = significant at the 10% level, ** = significant at the 5% level, and *** = significant at the 1% level. Columns (1) and (4) repeat results from Table 4 based on a specification without income effect, while the other columns consider a specification with income effects. The dependent variable in all specifications is the three-year growth rate in real wage earnings. The independent variables of interest are three-year growth rates in the marginal net-of-tax rate and virtual income, instrumented using mechanical variation in those variables created by tax reforms. All specifications include controls for labor market experience, age, gender, marital status, kids, place of residence, educational attainment, industry, local unemployment rate, and year fixed effects. Regressions are weighted by labor income and restricts the sample to individuals with positive labor income (in addition to the sample restrictions described in section 3). "Splines" refer to a flexible piecewise linear functional form with 10 components.

Table 6. Elasticities of Capital Income vs. Labor Income

Pre-reform income controls	Labor income (1)	Negative capital income (2)	Positive capital income (3)
No pre-reform income controls	-0.189*** (0.002)	-0.084*** (0.009)	0.081*** (0.024)
Log base-year (period s) income	0.060*** (0.002)	-0.103*** (0.007)	0.106*** (0.024)
Splines of log base-year (period s) income	0.044*** (0.002)	-0.127*** (0.005)	0.135*** (0.025)
Splines of log s-1 income and log deviation between s-1 and s incomes	0.049*** (0.002)	-0.120*** (0.005)	0.113*** (0.024)
Splines of log s-1 income and splines of log deviation between s-1 and s incomes	0.046*** (0.002)	-0.117*** (0.005)	0.125*** (0.024)
Splines of log s-2 income and log deviations between s-2 and s-1 incomes and between s-1 and s incomes	0.058*** (0.002)	-0.067*** (0.009)	0.097*** (0.024)
Number of observations	31,215,140	27,125,664	4,837,538

Notes: The table shows elasticity estimates based on 2SLS regressions, where standard errors (shown in parentheses) are clustered by individual. * = significant at the 10% level, ** = significant at the 5% level, and *** = significant at the 1% level. The dependent variable is real labor income in column (1), negative capital capital income in absolute value in column (2), and positive capital income in column (3). The independent variable of interest is the three-year growth rate in the marginal net-of-tax rate on labor income in column (1), negative capital income in column (2) and positive capital income in column (3), each instrumented using the three-year growth rate in the simulated marginal net-of-tax rate under base-year behavior. All elasticities are based on specifications without income effects and without cross-effects between labor and capital income. All specifications include controls for labor market experience, age, gender, marital status, kids, place of residence, educational attainment, industry, local unemployment rate, and year fixed effects. Regressions are weighted by income (labor income in column (1), capital income in columns (2)-(3)). Labor income regressions restrict the sample to individuals with positive labor income, while capital income regressions drop individuals with zero capital income and individuals whose capital income changes sign between base and post year (in addition to the basic sample restrictions described in section 3). "Splines" refer to a flexible piecewise linear functional form with 10 components.

Table 7. Labor and Capital Income Elasticities: Small vs. Large Reforms

	Panel A. Labor income			Panel B. Positive capital income		
	All reforms (1984-2005) (1)	1987 reform (1984-1990) (2)	Post-1987 reforms (1991-2005) (3)	All reforms (1984-2005) (4)	1987 reform (1984-1990) (5)	Post-1987 reforms (1991-2005) (6)
Pre-reform income controls						
No pre-reform income controls	-0.189*** (0.002)	-0.182*** (0.002)	-0.192*** (0.004)	0.081*** (0.024)	0.131*** (0.021)	0.124*** (0.032)
Log base-year (period s) income	0.060*** (0.002)	0.112*** (0.002)	0.043*** (0.004)	0.106*** (0.024)	0.137*** (0.023)	0.076** (0.032)
Splines of log base-year (period s) income	0.044*** (0.002)	0.104*** (0.002)	0.023*** (0.004)	0.135*** (0.025)	0.151*** (0.024)	0.109*** (0.035)
Splines of log s-1 income and log deviation between s-1 and s incomes	0.049*** (0.002)	0.111*** (0.002)	0.025*** (0.004)	0.113*** (0.024)	0.155*** (0.023)	0.094*** (0.032)
Splines of log s-1 income and splines of log deviation between s-1 and s incomes	0.046*** (0.002)	0.115*** (0.002)	0.018*** (0.004)	0.125*** (0.024)	0.153*** (0.023)	0.091*** (0.031)
Splines of log s-2 income and log deviations between s-2 and s-1 incomes and between s-1 and s incomes	0.058*** (0.002)	0.122*** (0.002)	0.034*** (0.004)	0.097*** (0.024)	0.157*** (0.023)	0.84*** (0.028)
Number of observations	31,215,140	11,799,628	19,415,512	4,837,538	1,756,743	3,080,795

Notes: The table shows elasticity estimates based on 2SLS regressions, where standard errors (shown in parentheses) are clustered by individual. * = significant at the 10% level, ** = significant at the 5% level, and *** = significant at the 1% level. Columns (1)-(3) consider labor income as the dependent variable, while columns (4)-(6) consider positive capital income as the dependent variable. Columns (1) and (4) include the full data period (1984-2005) and repeat results shown in Table 6. Columns (2)-(3) and (5)-(6) split the data into a period with large tax reform variation (1984-1990) and a period with smaller tax reform variation (1991-2005). All specifications are otherwise identical to those described in Table 6.

Table 8. Labor Income Elasticities: Own-Tax Effect and Cross-Tax Effect with Capital Income

	Panel A. All Individuals		Panel B. Wage earners		Panel C. Self-employed	
	Own-tax elasticity	Cross-tax elasticity	Own-tax elasticity	Cross-tax elasticity	Own-tax elasticity	Cross-tax elasticity
Pre-reform income controls	(1)	(2)	(3)	(4)	(5)	(6)
No pre-reform income controls	-0.114*** (0.003)	-0.099*** (0.002)	-0.112*** (0.003)	-0.010*** (0.002)	-0.141*** (0.031)	-0.092*** (0.029)
Log base-year (period s) income	0.065*** (0.002)	0.003 (0.002)	0.062*** (0.003)	0.003* (0.002)	0.116*** (0.035)	-0.018 (0.024)
Splines of log base-year (period s) income	0.049*** (0.003)	0.004*** (0.002)	0.047*** (0.002)	0.004*** (0.001)	0.086** (0.040)	-0.022 (0.026)
Splines of log s-1 income and log deviation between s-1 and s incomes	0.056*** (0.003)	0.004*** (0.002)	0.053*** (0.003)	0.005*** (0.002)	0.111*** (0.031)	-0.036 (0.029)
Splines of log s-1 income and splines of log deviation between s-1 and s incomes	0.064*** (0.003)	0.006*** (0.002)	0.062*** (0.003)	0.007*** (0.002)	0.101*** (0.037)	-0.041 (0.028)
Splines of log s-2 income and log deviations between s-2 and s-1 incomes and between s-1 and s incomes	0.067*** (0.003)	0.003* (0.002)	0.064*** (0.003)	0.005*** (0.002)	0.121*** (0.038)	-0.033 (0.031)
Number of observations	31,158,640		29,538,050		1,620,590	

Notes: The table shows elasticity estimates based on 2SLS regressions, where standard errors (shown in parentheses) are clustered by individual. * = significant at the 10% level, ** = significant at the 5% level, and *** = significant at the 1% level. The dependent variable in all regressions is the three-year growth rate in real wage earnings. The independent variables of interest are the three-year growth rate in the marginal net-of-tax rate on labor income (own-tax effect) and the three-year growth rate in the marginal net-of-tax rate on capital income (cross-tax effect). Both of these marginal net-of-tax rates are instrumented using three-year growth rates in simulated marginal net-of-tax rates under base-year behavior. All elasticities in the table are based on specifications without income effects. Regressions include taxpayers with non-zero capital income (positive or negative), and are otherwise based on the same sample restrictions and include the same controls as the previous specifications for labor income (as in Tables 4, 6, and 7).

Chapter 4

**Taxation and International Migration of Top
Earners: Evidence from the Foreigner Tax Scheme
in Denmark**

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Taxation and International Migration of Top Earners: Evidence from the Foreigner Tax Scheme in Denmark*

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Abstract

This paper analyzes the effects of income taxation on the international migration of top earners using the Danish preferential foreigner tax scheme. This scheme, introduced in 1991, allows immigrants with high earnings (above 103,000 Euros per year as of 2009) to be taxed at a flat rate of 25% for a duration of three years instead of the regular progressive schedule with a top marginal tax rate of 59%. Using population wide Danish administrative tax data, we show that the scheme doubled the number of highly paid foreigners in Denmark relative to slightly less paid ineligible foreigners, which translates into a very large elasticity of migration with respect to the net-of-tax rate in excess of one. There is bunching of earnings at the scheme eligibility threshold, evidence of a significant but quantitatively very small response along the intensive earnings margin (work effort or earnings manipulation through tax avoidance). There is also evidence of sharp bunching of durations of stay at the three year duration limit which translates into a significant but quantitatively small intensive duration response. In the end, the migration elasticity is much more larger than the conventional within country elasticity of earnings with respect to the net-of-tax rate. Hence, preferential tax schemes for highly paid workers could generate very harmful tax competition across European countries and severely limit the ability of European governments to use progressive taxation.

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1 Introduction

Tax-induced international mobility of talent is a controversial public policy issue, especially when tax rates differ substantially across countries and migration barriers are low as in the case of the European Union. High tax rates on highly paid workers may induce such workers to migrate to countries where the tax burden for top earners is lower. This can limit the ability of governments to raise taxes using progressive taxation by creating harmful tax competition. This issue is further exacerbated by the development of special favorable tax treatment reserved to highly skilled or highly compensated foreigners in a number of European countries.¹ As a result, the mobility response to tax rates looms even larger in the European policy debate on tax progressivity than traditional within-country labor supply responses.

While an enormous empirical literature has studied labor supply and taxable income responses to taxation (as surveyed by, e.g., Blundell and MaCurdy 1999 for labor supply, and Saez, Slemrod, and Giertz 2011 for taxable income), there is very little empirical work on the effect of taxation on the spatial mobility of individuals, and especially mobility among high-skilled workers. A small literature has considered the mobility of people across local jurisdictions *within* countries, including Kirchgassner and Pommerehne (1996) and Liebigh et al. (2007) on mobility across Swiss Cantons in response to Canton taxes, Feldstein and Wrobel (1998) and Bakija and Slemrod (2004) on mobility across U.S. states in response to state income and inheritance taxes, and Meyer (2000) on mobility across U.S. states in response to state welfare programs. However, empirical work on the effect of taxation on international mobility appears to be virtually non-existent² partly due to lack of micro data with citizenship information and challenges in identifying causal tax effects on migration. A recent exception is Kleven, Landais, and Saez (2010) who analyze the labor market for professional football players across 14 European Union countries and find compelling evidence of tax-induced mobility responses. However, a concern is that football players might be substantially more mobile than other highly skilled workers. In that case, the significant migration responses of Kleven, Landais, and Saez (2010)

¹Besides Denmark which we will analyze in this paper, Netherlands, Portugal, Spain, Switzerland also have preferential tax schemes for foreign workers that are significant.

²There is a very large literature on the effects of capital taxation on multinational corporations and international capital mobility (surveyed by, e.g., Gordon and Hines 2002). There is also an enormous literature on wage differentials and international migration (surveyed by, e.g., Borjas 1999), and some work on how international migration is affected by the generosity of social insurance and social welfare programs (e.g. Borjas 1999b).

would not be informative about the migration responses of high-skilled workers in general.³ This paper breaks new ground on this issue by analyzing the migration effects of a special tax scheme that applies to highly compensated foreign workers in Denmark regardless of occupation or industry.

In 1992, Denmark enacted a preferential tax regime for foreign researchers and high-income foreigners in all other professions, who sign contracts for work in Denmark after June 1, 1991. Under this scheme, the income tax rate is reduced to a flat rate of 25% (30% before 1996) for a total period of up to 3 years. Except for researchers, eligibility for this tax scheme requires an annual income above a certain threshold, which was set to 511,200 Danish kroner (about 69,000 Euros) when the scheme was introduced in 1992 and indexed to average earnings growth in subsequent years. As of 2009, the earnings threshold is about 103,000 Euros corresponding roughly to the 99th percentile of the distribution of individual earnings in Denmark.⁴ This scheme is much more generous than the regular tax system, which imposes a top marginal tax rate of 59% above a relatively modest annual income of 350,000 kroner (47,000 Euros). Absent the special tax scheme, workers with earnings above the scheme threshold would face average income tax rates around 55%, more than twice as high as the scheme rate. When the 3 years of preferential tax treatment have been used up, the taxpayer will be subject to the ordinary tax scheme imposing very high tax rates.⁵

This unusual piece of tax policy implies large discontinuities in tax liability depending on the contract start date (before and after June 1, 1991), duration of stay in Denmark (3-year and 7-year rules), earnings level (earnings eligibility threshold), and occupation (researchers versus other professions). Hence, the reform creates very large quasi-experimental variation along several different dimensions, and provides a very powerful way of identifying the effect of taxation on migration. It is very unusual to have large discontinuities in tax liability (as opposed to discontinuities in marginal tax rates) in tax systems. In this paper, we explore

³In particular, top footballers are young workers who earn most of their lifetime income over a short time period, and their profession involves relatively little country-specific capital. Furthermore, Kleven, Landais, and Saez (2010) do not have earnings level data on footballers, making it difficult to control for incidence effects of taxes on wages.

⁴The scheme also applies to Danish citizens, who have been abroad with no Danish source earnings for a period of at least 3 years.

⁵In the original law, after 7 years of residence in Denmark, a worker who had benefited from the preferential tax scheme would be subject to a claw-back whereby ordinary tax rates applied retroactively to the entire income earned during the period of preferential tax treatment. For a high-income worker, this rule implied a very large retroactive tax bill after 7 years of residence. The rule was eliminated for researchers in 1998 and substantially relaxed for all other professions in 2002, so that today the retro-active tax applies to very few workers.

the different aspects of the tax scheme using quasi-experimental techniques such as bunching approaches and difference-in-differences. For this analysis, we have access to administrative data including the universe of tax and payroll records for the entire population of Danish residents (Danish citizens and foreigners) since 1980. The data includes detailed information about citizenship, immigration history, income and tax variables, labor market variables, and socio-demographic information. It also contains specific information for all scheme beneficiaries. The data were specifically prepared by Statistics Denmark for our research project and securely accessed through a server at the Centre for Economic and Business Research (CEBR).

Our analysis of the Danish tax scheme for foreigners yields two main results. First and most important, we obtain compelling evidence that the scheme had a very large effect on the number of highly paid foreigners in Denmark. The number of foreigners paid above the eligibility threshold almost doubles relative to the number of foreigners paid slightly below the threshold after the scheme is introduced. This effect builds up in the first five years of the scheme and remains stable afterwards. This overall effect implies that the overall elasticity of migration with respect to the net-of-tax rate is very large, in excess of one. The resulting tax revenue maximizing rate for highly paid foreigners is therefore relatively small and around 40%. Taking into account all other taxes, including the VAT and payroll taxes, this implies that the very low 25% flat rate in Denmark is actually very close to the revenue-maximizing tax rate on high-income foreigners. Hence, it can be desirable from a single country perspective to adopt such preferential schemes for highly paid foreigners. At the same time, those schemes impose negative fiscal externalities on other countries and are detrimental to global welfare. This tension between country welfare and global welfare in tax policy making has loomed large in the public debate for a long time, but our paper provides for the first time compelling evidence that this is indeed a major tax policy issue. Absent coordination, it is conceivable that many countries would start adopting and extending such schemes which could unravel tax progressivity in Europe.

Second, using a simple theoretical framework, we decompose the global elasticity described above into the three underlying margins: the extensive migration margin, the intensive duration-of-stay margin, and the intensive earnings margin which includes labor supply adjustment and earnings manipulation through tax avoidance to qualify for the scheme threshold. Overall, the extensive migration margin accounts for over 80% of the total elasticity. The notch created by the earnings eligibility threshold does create a highly visible piling up in the density of earnings

for foreigners at the eligibility threshold (relative to controls) but this piling up is quantitatively extremely small relative the very large notch in the budget set. Using the methodology developed by Kleven and Waseem (2011), we find that the implied elasticity of earnings with respect to the marginal net-of-tax rate is less than .01. The kink in the life-time budget created by 3-year duration limit of the scheme does create sharp bunching in the density of duration of highly paid foreigners at the 3-year cutoff (relative to controls), but this bunching is also quantitatively small. Using the methodology developed by Saez (2010), we find that this bunching translates into an elasticity of duration of stay (conditional on being in the scheme) with respect the net-of-tax rate around .1. Hence, this decomposition analysis reinforces our earlier finding that the migration elasticity is an order of magnitude larger than the elasticity of reported earnings, which has been the primary focus in the empirical literature on behavioral responses to individual taxation. This also makes it particularly important for advanced economies and particularly European countries to start creating and sharing data on labor migration, especially among highly skilled workers, to monitor the extent of tax induced mobility.

Finally, it is important to note that our results are based on a single small country, Denmark, with a specific language, culture, location and climate. It is a country where almost everybody speaks English and it provides a high quality of life and government services. Furthermore, it is a small and relatively homogeneous country with no strong tradition of immigration. Hence, relative to other countries with stronger traditions of immigration, such as the United States, Denmark starts from a very small base of highly paid foreigners. As a result, in terms of external validity, the extremely large elasticity estimated for Denmark might not carry over to larger countries starting with a higher base of foreign workers. This is closely related to the important point made by Kanbur and Keen (2003) in the context of capital mobility that tax heavens tend to be small countries because small countries start with small bases and hence can large elasticities by granting favorable tax treatment to foreigners. We hope that our pioneering work can be applied to the many other countries, which have adopted such schemes if/when data become available in the future.

The paper is organized as follows. Section 2 presents key features of the Danish tax scheme, proposes a simple theoretical framework, and describes the administrative data we use. Section 3 presents the empirical analysis, while section 4 concludes by discussing policy implications and future work on analyzing spillovers.

2 Context, Conceptual Framework, and Data

2.1 The Danish Tax Scheme

In 1992, Denmark enacted a preferential tax scheme for foreign researchers and high-income foreigners in all other professions, who sign contracts for employment in Denmark after June 1st, 1991. The scheme is commonly known in Denmark as the *Researchers' Tax Scheme*. When the scheme was first introduced, it offered a flat income tax rate of 30% in lieu of the regular progressive income tax with a top marginal tax rate of more than 60% and an average tax rate on high-income workers around 55%. The scheme rate was reduced to 25% in 1995, but at the same time a labor market contribution was added, leaving the total scheme rate roughly unchanged around 30%.

The scheme can be used for a total period of up to 36 months after which the taxpayer becomes subject to the ordinary income tax schedule. The 36 months do not have to be taken together, but can be divided into any number of spells over an unlimited period of time.⁶ As we discuss in more detail in the next section, this form of duration dependence creates a discrete jump in *marginal* lifetime tax liability with respect to duration at the 3-year cutoff—a *kink* in the lifetime budget set as a function of duration. Besides this form of duration dependence, the initial scheme design stipulated that a worker who stayed in Denmark for another 48 months after having benefitted from the special tax scheme would face a claw-back equal to the entire tax savings during the period of preferential tax treatment. For a worker who had benefitted from the scheme for the maximum 3-year period, this rule implied a very large retroactive tax bill after 7 years of residence. This form of duration dependence creates a discrete jump in *total* lifetime tax liability at the 7-year cutoff—a *notch* in the lifetime budget set as a function of duration. The duration notch was eliminated for researchers in year 2000 and substantially relaxed for all other professions in year 2002, so that today it applies to very few workers.

There are two key requirements to become eligible for the preferential tax scheme. The first requirement is that the taxpayer has been recruited abroad and not been tax liable in Denmark in the 3 years prior to going on the scheme.⁷ Until very recently, citizenship played no formal role

⁶After 2008 (outside our data period), an additional scheme option was introduced whereby eligible workers can choose between the standard scheme rate of 25% for 36 months and a higher scheme rate of 33% for 60 months (a labor market contribution comes in addition to both of those rates). Reports suggest that the take-up of the newly introduced 60-month scheme option has been very low.

⁷For taxpayers who split scheme take-up into several spells, the 3-year eligibility requirement applies to each spell separately. But in assessing whether a taxpayer has been tax liable in Denmark prior to a given scheme

in determining eligibility, and therefore Danish citizens who had been living and paying taxes abroad for at least 3 years could also apply for the scheme. Those rules have now been changed such that Danish citizens must be foreign tax residents for 10 years in order to become eligible for the scheme, but this reform lies outside our current data period. The second requirement is that, unless the worker qualifies as a researcher, annual employment earnings must be at least 765,600 Danish kroner (about 103,000 Euros) in 2009 prices. The threshold grows roughly at the same rate as the average earnings level in Denmark. This cutoff lies between the 99th and 99.5th percentile of the Danish employment earnings distribution. As the preferential scheme rate applies to all units of earnings conditional on eligibility, the earnings requirement creates a discrete jump in *total* annual tax liability at the cutoff—a *notch* in the annual budget set as a function of earnings.

In terms of administration, the scheme treatment had to be requested by the employer. Hence, the employer has to show the tax authorities that the level of earnings is above the eligibility threshold and that other qualifying requirements are met. Importantly, the threshold for eligibility applies only to earnings with the specific employer requesting the scheme. Having other sources of income or earnings does not help qualify. The threshold of eligibility must be met on an annualized basis. Hence for a contract of 6 months, the eligibility threshold is that the 6 months of pay must be at least half of the annual threshold, i.e., 51,150 Euros (as of 2009). If the scheme beneficiary has other income besides scheme earnings, that income is taxed according to the standard progressive income tax schedule and completely independently of scheme earnings. In other words, scheme earnings are effectively taxed at a flat rate completely independently of the other circumstances of the individual.

To summarize, the special Danish tax scheme creates the following tax variation. First, the scheme introduced much lower tax rates on a specific sample of people (high-income foreigners; not tax liable in Denmark 3 years prior) at a given point in time. This variation provides an ideal setting for a difference-in-differences analysis of migration effects. Second, the scheme introduced a 3-year duration kink among those who migrate to Denmark, providing sharp quasi-experimental variation that can be used to study the effects of taxation on duration of stay. Third, the scheme originally introduced a 7-year duration notch, which in principle offer more quasi-experimental variation to study duration. However, since this rule was eliminated

spell, the time spent in Denmark under prior scheme spells are not counted.

for most taxpayers not long after the first scheme participants could potentially hit the 7-year notch, we do not have enough observations around the notch to study this precisely. Fourth, the scheme introduced an earnings notch creating very strong incentives for foreigners to have earnings above the income cutoff. This creates compelling quasi-experimental variation to study earnings responses to taxation among foreign migrants.

2.2 Conceptual Framework

We consider a two-country model consisting of a destination country for potential migrants (Denmark) denoted by d and a home country of migrants (rest of the world) denoted by h . The destination country is small and its tax policy therefore does not impact the equilibrium in the rest of the world.

Consider a migrant who spends Y years in Denmark out of a total working life of \bar{Y} years. The utility obtained while in Denmark is given by

$$u^d = \int_0^Y \left\{ z_y^d - T_f^d(z_y^d, y) - nh_z \left(\frac{z_y^d}{n} \right) \right\} dy - \left\{ \mu + \nu h_Y \left(\frac{Y}{\nu} \right) \right\} \cdot 1(Y > 0), \quad (1)$$

where z_y^d is before-tax earnings in Denmark in year y , $T_f^d(\cdot)$ is the tax schedule for foreigners in Denmark (which may depend on both earnings and the year of Danish residence), $h_z(\cdot)$ is disutility of hours worked, n is labor market ability, μ is a moving cost, $h_Y(\cdot)$ is a disutility of spending Y years in Denmark, and the parameter ν captures preferences for Denmark. The disutility functions h_z, h_Y are increasing and convex, so that larger values of the parameters n, ν are associated with lower marginal disutility (of working and being in Denmark, respectively).

The utility outside of Denmark is given by

$$u^h = \int_Y^{\bar{Y}} \left\{ z_y^h - T^h(z_y^h) - nh_z \left(\frac{z_y^h}{n} \right) \right\} dy = (\bar{Y} - Y) \left(z^h - T^h(z^h) - nh_z \left(\frac{z^h}{n} \right) \right), \quad (2)$$

where z_y^h is before-tax earnings at home in year y and $T^h(\cdot)$ is the annual tax schedule in the home country (which does not depend on the number of years spent there). The second equality uses that, because annual tax liability at home is not a function of the year of residence y , the earnings choice is the same in every period ($z_y^h = z^h$).

Total lifetime utility is given by $u^d + u^h$, which is maximized with respect to time spent in Denmark Y , earnings in Denmark in each period z_y^d , and earnings at home z^h . Choosing time

spent in Denmark consists of an extensive margin ($Y = 0$ vs. $Y > 0$) and an intensive margin (size of Y conditional on $Y > 0$).

There is heterogeneity in three dimensions: moving costs μ , preferences for Denmark ν , and labor market ability n . There is a smooth distribution of potential migrants characterized by the density function $f(\mu, \nu, n)$ on the domain $D = (0, \infty) \times (0, \infty) \times (0, \infty)$.

As a benchmark, we start from a smooth (continuously differentiable) tax schedule on foreigners in Denmark. Denoting the marginal tax rate on earnings by $\tau_f^d(z_y^d, y) \equiv \frac{\partial T_f^d(z_y^d, y)}{\partial z_y^d}$, foreigner earnings in year y in Denmark are characterized by

$$h'_z\left(\frac{z_y^d}{n}\right) = 1 - \tau_f^d(z_y^d, y). \quad (3)$$

Duration in Denmark Y (conditional on $Y > 0$) is characterized by

$$h'_Y\left(\frac{Y}{\nu}\right) = \left[z_Y^d - T_f^d(z_Y^d, Y) - nh_z\left(\frac{z_Y^d}{n}\right)\right] - \left[z^h - T^h(z^h) - nh_z\left(\frac{z^h}{n}\right)\right]. \quad (4)$$

The left-hand side is the marginal disutility of time spent in Denmark, while the right-hand side is the marginal consumption increase net of disutility working from time spent in Denmark instead of at home. If the tax schedule were independent of year of residence (in which case $T_f^d(z_Y^d, Y)$ and hence z_Y^d would be independent of Y), then the right-hand side would be constant in Y . For the intensive-margin choice of Y to be positive, the right-hand side must be positive at zero duration, which depends on tax liability at zero duration ($T_f^d(z_0^d, 0)$ compared to $T^h(z^h)$) and on earnings—and therefore marginal tax rates—at zero duration ($\tau_f^d(z_0^d, 0)$ compared to $\tau^h(z^h)$).

Let us now consider the extensive-margin choice of whether to move to Denmark at all. Moving to Denmark requires that lifetime utility $u^d + u^h$ is higher under $Y > 0$ (given optimal choices of Y and z_y^d as characterized above) than under $Y = 0$, which implies

$$\mu \leq \int_0^Y \left\{ z_y^d - T_f^d(z_y^d, y) - nh_z\left(\frac{z_y^d}{n}\right) \right\} dy - \nu h_Y\left(\frac{Y}{\nu}\right) - Y \left(z^h - T^h(z^h) - nh_z\left(\frac{z^h}{n}\right) \right), \quad (5)$$

where $z_y^d = z_y^d(n)$ is determined by (3) and $Y = Y(n, \nu)$ is determined by (4). We denote the right-hand side of (5) by $\mu^* = \mu^*(n, \nu)$. The total number of people migrating to Denmark is then given by

$$N = \int_0^\infty \int_0^\infty \int_0^{\mu^*(n, \nu)} f(\mu, \nu, n) d\mu d\nu dn. \quad (6)$$

The total amount of time spent by migrants in Denmark is given by

$$S = N \cdot E[Y] = \int_0^\infty \int_0^\infty \int_0^{\mu^*(n,\nu)} f(\mu, \nu, n) Y(n, \nu) d\mu d\nu dn, \quad (7)$$

where $E[Y]$ is the average duration among those who migrate to Denmark. The total amount of earnings created by migrants in Denmark is given by

$$N \cdot E[Y \cdot z^d] = \int_0^\infty \int_0^\infty \int_0^{\mu^*(n,\nu)} \int_0^{Y(n,\nu)} f(\mu, \nu, n) z_y^d(n) dy d\mu d\nu dn, \quad (8)$$

where $z^d \equiv \left[\int_0^Y z_y^d dy \right] / Y$ is average earnings per year in Denmark and $E[Y \cdot z^d]$ is average earnings over the entire stay in Denmark among all migrants.

We define by

$$e = \frac{1 - t^d}{S} \frac{\partial S}{\partial(1 - t^d)}$$

the global elasticity of migrants S with respect to the net-of-tax rate $1 - t^d$ where t^d is the average tax rate in Denmark. This elasticity is defined for a given maximum duration y^* and given tax rates in the home country. Naturally, e is likely to increase with y^* as it is likely to be small for small y^* (and actually zero when $y^* = 0$). For very large y^* , the elasticity converges to \bar{e} which is the conventional elasticity of migration with respect to the net-of-tax rate using in optimal tax models with migration such as Mirrlees (1982).

We define by

$$e^A = \frac{1 - t^d}{N} \frac{\partial N}{\partial(1 - t^d)}$$

the elasticity of arrivals N with respect to the net-of-tax rate $1 - t^d$ where t^d is the average tax rate in Denmark. This capture the extensive margin of arrivals.

We will estimate the effect of taxation on all three dimensions of behavior in eqs. (6)-(8): the number of migrants, average duration of stay, and average earnings among those who migrate to Denmark. We do this using a reform that introduced sharp changes in the tax schedule on foreigners $T_f^d(z_y^d, y)$. Before the reform, the tax schedule $T_f^d(z_y^d, y)$ was the same as for domestic residents, a standard piecewise linear function of earnings with no duration dependence. After the reform, the tax schedule $T_f^d(z_y^d, y)$ had much lower rates than the domestic schedule and a very different structure. The new schedule offers a low flat tax rate under two eligibility requirements. First, the schedule is dependent on duration of stay in Denmark with low tax rates applying to years $y \leq y^*$ and high tax rates (standard domestic rates) applying to years $y > y^*$. As the higher rates apply only to the period after y^* , this policy creates a discrete jump

in the *marginal tax* with respect to duration Y (which is given by $T_f^d(z_Y^d, Y)$) and therefore a *kink* in the budget set with respect to duration Y . Second, eligibility for the low flat tax in a given year requires earnings z_y^d above a cutoff z^* , with standard domestic tax rates applying to immigrants with earnings below z^* . As the low flat tax applies to all units of income for those with earnings above the cutoff z^* , this piece of policy creates a discrete jump in *tax liability* with respect to earnings and therefore a *notch* in the budget set.

Figure 1 illustrates behavioral responses to the duration kink (Panel A) and the earnings notch (Panel B). Panel A considers individuals at a given ability n (high enough for earnings to be above the earnings notch z^*), but with different preferences ν for staying in Denmark. Indifference curves represent preferences over duration Y and "net consumption" defined as lifetime consumption net of disutility of working. The slope of the indifference curves is given by $h'_Y(\frac{Y}{\nu})$, corresponding to the left-hand side of equation (4). The slope of the budget line (defined with respect to net consumption) is given by the right-hand side of equation (4). Starting from a duration-independent tax schedule (so that $T_f^d(z_Y^d, Y)$ does not change with Y and therefore z_Y^d does not change with Y), the slope of the budget line is constant in Y . In this case, the durations among immigrants is smoothly distributed reflecting the smooth distribution of ν . Suppose now that tax liability $T_f^d(z_y^d, y)$ is increased after year y^* (at a given level of earnings). This creates a kink in the budget line at $Y = y^*$ as the slope on the right-hand side of equation (4) jumps. This kink produces bunching of individuals whose durations were falling into a segment $[y^*, y^* + \Delta y^*]$ before the kink was introduced. Individual L has the shortest pre-kink duration (smallest ν) among those who bunch at the kink; this individual chooses duration y^* both before and after the reform. Individual H has the longest pre-kink duration (largest ν) among those who bunch at the kink; this individual chooses duration $y^* + \Delta y^*$ before the tax reform and duration y^* after the reform. Every individual between L and H locates at the kink point, which creates total bunching $B = \int_{y^*}^{y^* + \Delta y^*} g_0(y) dy$ where $g_0(\cdot)$ denotes the counterfactual (pre-kink) duration distribution. Based on estimates of $g_0(y)$ and B , this relationship yields the duration response Δy^* which can be used to back out the compensated elasticity of duration with respect to the tax rate following the method developed in Saez 2010. Saez (2010) shows that the intensive elasticity e^I can be approximated by the following equation:

$$\frac{\Delta y^*}{y^*} \simeq e^I \frac{\Delta \tau}{1 - \tau}$$

where t^* is the location of the kink equal to 3 years here and $\Delta \tau$ is the change in marginal

incentive (in the life-time budget) and τ the current marginal tax rate (in the life-time budget).

Panel B considers the implications of the earnings notch. This diagram considers earnings choices z_y^d at a point in time $y < y^*$ for individuals with different abilities n . Indifference curves represent preferences over labor supply z_y^d/n and consumption c_y^d at a point in time in Denmark (the slope of which is $h'_z(z_y^d/n)$) and the budget set is given by $c_y^d = z_y^d - T_f^d(z_y^d, y)$. For simplicity of exposition, we start from a linear tax schedule before the reform (although in practice the pre-existing schedule was piecewise linear). Under the baseline tax system, there is a smooth distribution of earnings reflecting the smooth distribution of n . Now a preferential flat tax is introduced for foreigners, conditional on having earnings above a cutoff z^* . This creates an upward notch in the budget set at z^* , which produces bunching at z^* combined with a hole in the density distribution in an interval $[z^* - \Delta z^*, z^*]$. Individual L has the lowest pre-notch income (lowest n) among those who locate at the notch point; this individual chooses earnings $z^* - \Delta z^*$ before the reform and is exactly indifferent between $z^* - \Delta z^*$ and the notch point z^* after the reform. Individual H has the highest pre-notch income (highest n) among those who locate at the notch point; this individual chooses earnings z' before the reform and earnings z^* after the reform. Individuals who were initially between z' and z^* move to the upper bracket (strictly above z^*) such that no individuals locate between $z^* - \Delta z^*$ and z^* under the notched schedule. The total amount of bunching is given by $B = \int_{z^* - \Delta z^*}^{z'} h_0(z) dz$ where $h_0(\cdot)$ denotes the counterfactual (pre-notch) earnings distribution. A technical complication is that estimates of B and $h_0(z)$ are not sufficient to reveal Δz^* as we do not know z' .⁸ A lower bound on the size of the earnings response can be obtained by assuming $z' \simeq z^*$ (corresponding to an assumption that the uncompensated elasticity is not too large). Based on an estimate of Δz^* obtained in this way, it is possible to back out the compensated elasticity of earnings with respect to the marginal tax rate using the method set out by Kleven and Waseem (2011).⁹

⁸This is an issue specific to *upward* notches and does not arise in the case of downward notches (Kleven and Waseem 2011).

⁹When implementing this strategy empirically, the above bunching formula should be modified to account for the fact that the observed below notch density will not be completely empty on the interval $[z^* - \Delta z^*, z^*]$ due to the presence of frictions. To deal with this, Kleven and Waseem (2011) propose a bunching formula of the form $B = \int_{z^* - \Delta z^*}^{z^*} [h_0(z) - h(z)] dz$ where $h(z)$ is the observed below notch earnings density (which captures those who are stuck on the segment $[z^* - \Delta z^*, z^*]$ due to frictions).

2.3 Administrative Tax Data

The data we use in this paper comes from an administrative dataset including the Universe of tax and payroll records for all resident individuals in Denmark including both Danish citizens and foreigners since 1980. The data include detailed information about income and tax variables, labor market variables, and socio-demographic information. Most importantly, the data contains very specific citizenship and migration information. Each individual working in Denmark must receive a personal identification number (CPR) in order to pay withholding taxes, rent an apartment, etc. The application for a CPR number contains detailed questions about citizenship and date of entry in Denmark, as well as about the country of in-migration. The registry administration updates this information in case an individual leaves the country, but this update can take a few years, and therefore we only have precise information about in and out migration until 2005. The data also contains very detailed information for all scheme beneficiaries with precise information on starting and ending dates of labor contracts. Unfortunately, this information was not computerized for the first years of the scheme and therefore we do not have individual wage information available for scheme beneficiaries for the years 1991 to 1994 included.

2.4 Summary Statistics

Number and Composition of Scheme Workers.

Figure 2 reports the composition of beneficiaries of the tax scheme (excluding researchers) by country of citizenship (Panel A) across all years 1991 to 2006. Unsurprisingly, the vast majority of scheme workers come from advanced economies, 25% come from Nordic countries, 10% are Danish citizens (who qualify as others as long as they have not paid taxes in Denmark for 3 years or more), 19% from the United Kingdom or Ireland, 10% from Northern-America, and about 20% from Germany, France, and Benelux combined. The number coming from emerging countries or Eastern Europe and Russia is extremely modest.

Take-up Rate.

Figure 3 reports the take-up rate for the scheme among foreigners arriving in Denmark with (annualized) earnings above the eligibility threshold and who have not paid taxes in Denmark over the last 3 years. The take-up rate is high, around 70%, but still significantly below 100%

for a variety of reasons. First, companies have to file out an application form for each employee eligible for the scheme. It is conceivable that not all companies knew about the scheme or were willing to do the administrative paperwork. Second, some individuals may not have been willing to take-up the scheme perhaps because of the claw-back rule after 7 years. Third, individuals may not be fully aware of the existence of the scheme, or might come to Denmark as political refugees. Following the Dayton Peace Agreements in December 1995 for instance, there was an important influx of migrants from Bosnia-Herzegovina, who were for the most part political refugees and some of them being high-skilled workers, creating a drop in the take-up rate of the scheme in 1995-1996.

3 Empirical Evidence on Behavioral Responses

In this section, we first estimate in Section 3.1 the overall effect of the reform and summarize the effect with a global elasticity. In the following subsections and using the conceptual framework we developed earlier, we decompose the global effect into a migration extensive effect, intensive labor supply effect on the job, and intensive effect along the duration of stay margin.

3.1 Global Reduced-Form Elasticity

The most transparent way to test whether the Danish tax scheme had an impact is to plot the number of foreigners with earnings above the threshold overtime as we do in Figure 4. The series (denoted as treatment in the figure) shows that the number of highly paid foreigners was fairly stable around 800 from 1980 to 1990 before the scheme was implemented. After the scheme takes place in 1991, denoted as a vertical line in the figure, there is a steady increase in the number of highly paid foreigners. The number highly paid foreigners reaches over 3000 in 2006. Naturally, it is conceivable that the number of highly paid foreigners could have increased absent the scheme because of European Union labor market integration following the Single European Act signed in 1986 and implemented in 1992. The simplest way to control for such trends is to also plot series of the number of highly paid foreigners slightly below the threshold of eligibility for the scheme. Figure 4 therefore reports the number of foreigners in Denmark with earnings between 80% and 90% of the threshold (control 1) and with earnings between 90% and 99.5% of the threshold (control 2). Both series are normalized so that they match the treatment series in 1991 the year before the scheme was first implemented. Three lessons

emerge from those controls.

First, the control series follow very closely the treatment series before the scheme is implemented. All three series are remarkably stable providing credibility to our assumption that those groups of foreigners below the threshold are good control groups for the treatment group above the threshold. Second, after the scheme is implemented, the control groups series only increase modestly in the first 5 years. By 1995, the control series are virtually identical to 1990 levels while the treatment series have almost doubled. After 1995, the control series increase steadily over time but much more slowly than the treatment series. Indeed, after 1995, the treatment series are consistently about twice as high as the control series. Third, there might be a concern that highly paid foreigners above the threshold are displacing highly paid foreigners slightly below the threshold through intensive earnings responses as we described in the theory section. For example, a company that was planning to hire a foreigner just below the threshold might adjust the contract to require slightly more work for a slightly higher pay qualifying for the contract. It might also shift the form of compensation to increase cash compensation at the expense of fringe benefits or on the job perks that do not qualify toward the threshold. Such shifting should produce a dip in the number of foreigners just below the threshold relative to foreigners further down below the threshold. The very close and parallel pattern of evolution of both control groups (the one just below the threshold 90% to 99.5% and the one further down the threshold 80 to 90%) shows that this dip effect was not significant. We will come back to this issue in more detail when we analyze earnings densities for foreigners around the threshold in Section 3.2.

Figure 5 shows heterogeneity in responses by country of origin of foreign immigrants. It repeats Figure 4 but restricting foreigners to sub-groups of countries: Nordic Countries (Finland, Iceland, Norway, Sweden) in Panel A, English Speaking Countries (Australia, Canada, Ireland, New Zealand, UK, US) in Panel B. Continental Europe (all other European countries excluding Eastern Europe) in Panel C, and all other countries in Panel D. The figure shows that each country group experienced a large migration effect. However, there is heterogeneity across country groups: the response is somewhat smaller for Nordic countries and continental Europe and significantly larger for English speaking countries. Other countries (primarily Eastern Europe) show at first no response and then a very large response after 1996 when migration from Eastern Europe to Denmark was liberalized.

An additional concern with our analysis is that there might have been a widening of earnings inequality in Denmark since the 1990s. Such widening would have increased the number of workers paid above the threshold both among Danish natives and foreigners and hence led to a divergence in the number of foreigners in the treatment group and control groups even absent any scheme effect. The threshold for eligibility always lies between the 99th and the 99.5th percentile of the full earnings distribution among Danish adults with positive earnings. This concern is unlikely to be an issue in the case of Denmark as Kleven and Schultz (2011) have shown that top income shares have remained remarkably stable in Denmark since 1980. We address this issue with our data in two ways.

First, Figure 6 plots average earnings in various upper percentile groups both below and above the threshold among Danish citizens (normalized to 1 in 1990). It shows quite parallel trends for all groups confirming the findings of Kleven and Schultz (2011) that the upper tail of the earnings distribution did not widen in Denmark in recent decades.

Second, Figure 8, Panel A then plots the fraction of foreigners in various percentiles of the earnings distribution. We have a gap in 1991-1994 because the scheme related data does not provide scheme earnings for those years. Two important findings should be noted from the figure. First the fraction of foreigners in each percentile is very stable before 1991.¹⁰ Second, after 1991, the fraction of foreigners increases much more rapidly—both in absolute and percentage terms—in the the two top groups above percentile 99.5th where the scheme applies. Normalizing the fraction of foreigners to one in 1991, in Figure 8, Panel B, we see that the fraction of foreigners doubles from 1990 to 1995 above percentile 99th while it remains pretty stable below percentile 99th.

In conclusion, estimates based on the fraction of foreigners in various fixed percentiles of the distribution, which can fully control for changes in earnings dispersion deliver roughly the same doubling effect as our earlier estimates based on the increase in foreigners above the fixed (in real terms) threshold. Hence, if we use as identification assumption that, absent the scheme, the relative number of foreigners above the threshold would have increased at the same rate as the number of foreigners slightly below the threshold, we find an extremely large doubling effect of the scheme.

Table 1 summarizes the graphical evidence described above and gives more formal estimates

¹⁰The fraction of foreigners is higher, around 5%, above the 99.9th percentile, than in percentiles 96 to 99.9 where it is around 2 to 3%.

of the effect of the tax scheme on the total number of foreigners in Denmark. We estimate difference-in-difference models of the following form:

$$Y_{it} = \alpha_0 + \alpha_{tr} \cdot 1[i = 1] + \alpha_{rf} \cdot 1[t > 1991] + \alpha_{tr*rf} \cdot 1[i = 1] \cdot 1[t > 1991] + \nu_{it}$$

where Y_{it} is the total number of foreigners (weighted by duration) in income group $i = 0, 1$ present in Denmark in year t . Assuming that, absent the reform, the number of foreigners would have followed the same trend in the control ($i = 0$) and treatment ($i = 1$) group, the effect of the reform on the total number is identified by the coefficient α_{tr*rf} . We also compute the percentage increase in the total number of foreigners as $\frac{\alpha_{tr*rf}}{\alpha_0 + \alpha_{rf} + \alpha_{tr}}$, where $\alpha_0 + \alpha_{rf} + \alpha_{tr}$ is the counterfactual number of foreigners that would have prevailed in the treatment group after 1991 in the absence of a reform.

In column (1) we define control as the income group lying between 80% and 100% of the scheme threshold level, and treatment as the the income group above the threshold level. Pre-reform period is years 1980 to 1990 and post-reform is defined as years 1995-2000, in order to estimate the longer term effect of the reform. The estimated percentage increase in the number of foreigners is 90% and precisely estimated. In column (2), we focus on the short term effect by comparing years 1991-1995 to 1980-1990. The effect is still large and significant, with an estimated percentage increase of .65. In column (3), we estimate a placebo specification, where the treatment group is constituted of individuals with income between .9 and 1 times the threshold level, while the control group is constituted of individuals with income between .8 and .9 times the threshold level. This specification is also a test for the presence of shifting around the threshold in case of strong behavioral responses in terms of earnings or avoidance, since shifting should produce a dip in the number of foreigners just below the threshold relative to foreigners further down below the threshold. The coefficient α_{tr*rf} is negative but very small and not significant, which confirms the graphical evidence that shifting behaviors are second-order. In column (4), we use individuals with income above the 99.5-th percentile of the income distribution as treatment, and individuals with income between the 96-th and the 99-th percentile of the income distribution as control. The results are in line with column (1) and the estimated percentage increase in high-skilled foreigners implied by the reform is 111%. We then decompose the effect by regions of origin in column (5) to (8). As suggested by figure 5, the effect of the reform is stronger among high-skilled individuals from English-speaking countries,

with an estimated percentage increase of 112%.

Elasticity Estimate.

Based on the graphical evidence we have presented, we see that the number N of foreigners above the threshold increases by about 100% relative to the number of foreigners slightly to somewhat below the threshold. Our identification assumption is that all of this extra-increase is due to the introduction of the tax scheme. As discussed above, this assumption is validated by the pre-scheme data from 1980 to 1990 and with the post-scheme data when comparing various control groups below the threshold.

The average tax rate τ in the scheme is a flat 25% while the tax rate outside scheme is around 55%. This implies that the scheme produces an increase in the net-of-tax rate from 45% to 75%, i.e., a percent increase in the net-of-tax rate of $75/45-1=67\%$. Hence, the global elasticity e is estimated as:

$$e = \frac{1 - \tau}{N} \frac{\partial N}{\partial(1 - \tau)} \simeq \frac{1}{0.67} = 1.5.$$

This is a very large elasticity. This elasticity applies for the existing scheme with its three year duration. Conceivably, the elasticity for a scheme with indefinite duration could be even higher.

This large elasticity implies that the tax revenue maximizing rate is $\tau^* = 1/(1 + e) = 40\%$ using the conventional inverse elasticity formula. When taking into account the 20% VAT tax (that applies to only to the fraction of consumption subject to the VAT rate) and the 8% payroll tax, this shows that the 25% scheme tax is not very far from the tax revenue maximizing rate.

As we discussed in the theory section, the elasticity we have estimated can be due to (a) the extensive margin of migration to Denmark of highly skilled foreigners due to lower average tax rate of the scheme, (b) the intensive margin along the earnings level margin to take advantage of the scheme that creates a notch in the individual budget set at the threshold of eligibility, (c) the intensive margin along the duration of stay margin as the scheme lasts only three years.

3.2 Decomposing the Global Elasticity

Extensive Elasticity: Arrivals in Denmark.

Next, we analyze the response along the extensive margin, namely the migration to Denmark of highly paid foreigners. Figure 9 reports the number of foreigners with earnings above the scheme eligibility threshold (treatment series) arriving each year in Denmark from 1980 to 2006. As

control groups, it reports the number of foreigners arriving in Denmark with earnings between 80% and 90% of the threshold (control 1) and with earnings between 90% and 99.5% of the threshold (control 2). Both control series are normalized so that they match the treatment series in 1990 the year before the scheme was first implemented. The vertical line at year 1991 denotes the year the scheme was first implemented. This figure is quantitatively very similar to our initial graph with the absolute number of foreigners. It shows a slightly more than doubling of arrivals of foreigners above the threshold relative to foreigners below the threshold. This shows that the extensive arrival elasticity is even slightly higher than the global elasticity we estimated initially. The difference could be due to composition effects as people attracted to Denmark because of the scheme might have lower durations of stay than people coming to Denmark absent the scheme.

Intensive Elasticity along the Earnings Margin

In order to analyze the intensive elasticity along the earnings level margin, we exploit the fact that the scheme creates a notch in the individual budget set as eligibility kicks in above a fixed threshold. As we described in the theory section, behavioral responses to take advantage of the notch could be along the labor supply margin (working more to earn more and qualify for the scheme) or the tax avoidance margin (adjusting the form of compensation to increase cash compensation to qualify for the scheme at the expense of other compensation). Such behavioral responses should create a piling up in the earnings density of foreigners at the earnings threshold along with a gap just below the threshold.

To examine this, Figure 10 plots such densities before the scheme was introduced (1988-1990 in light grey) and after the scheme was introduced (1995-1997 in black). The vertical line denotes the threshold and the x-axis is normalized so that zero corresponds to the threshold. The density is smooth around the threshold before the scheme was introduced. After the scheme was introduced, the density is virtually identical below the threshold, but two differences appear above the threshold. First, the density is everywhere higher above the threshold consistent with a strong migration response at all earnings levels above the threshold. Second, there is clear bunching at the threshold, but with only a very small dip to the left of threshold. Even though the bunching is clearly visible in the figure and therefore provides very compelling evidence of a behavioral response, it is in reality very modest relative to the extremely large notch in budget set created by the scheme.

Using the methodology developed by Kleven and Waseem (2011), we can estimate that the spike at the notch point translates into an elasticity of earnings with respect to the marginal net-of-tax rate that is extremely small, less than .01. This estimation assumes iso-elastic utility with no income effects in which case the intensive earnings elasticity e with respect to the marginal net-of-tax rate is determined by

$$\frac{z^* - \Delta z^*}{z^*} = (1 + e) \frac{1 - \tau^s}{1 - \tau^d} - e \left(\frac{z^*}{z^* - \Delta z^*} \right)^{\frac{1}{e}},$$

where τ^s is the average tax rate under the scheme, τ^d is the average tax rate in the regular tax schedule, z^* is the earnings cutoff, and Δz^* is the earnings response by the marginal bunching individual as described in the theory section. The earnings response Δz^* can be determined as described in section 2.2 based on the amount of excess bunching at the notch relative to the local counterfactual density, absent the notch.

Using Figure 10, we have $\Delta z^* \simeq 10,000$ kroner as the excess mass at the notch point corresponds roughly to the density mass between the threshold and the threshold + 10,000 kroner. Moreover, we have $z^* \simeq 550,000$ kroner (scheme eligibility threshold in the mid-1990s shown in the figure) along with average income tax rates $\tau^d = 45\%$ below the cutoff and $\tau^s = 25\%$ above the cutoff. Using those numbers in the formula above, we obtain $e \simeq .004$. This elasticity is extremely tiny as the amount of bunching, although significant, is very modest relative to the enormous change in average tax rate of 20 points at the notch.¹¹ For example, even a modest elasticity of 0.1 would translate into a Δz^* of around 100,000 kroner, i.e., a massively large amount of bunching.

Naturally, it is possible that this elasticity is so tiny because of large frictions in the ability of highly paid foreigners to adjust their labor supply and earnings as in the theory proposed by Chetty (2011). In the particular case of the scheme, such an adjustment needs to be negotiated between the employee and the employer before hiring takes place and while the worker is still abroad as only new immigrants are eligible for the scheme. Hence, such an agreement might be difficult to set-up unless employers actively cooperate. As scheme workers are so few, employers may not want to systematically change their behavior to accommodate them as in the theory proposed by Chetty et al. (2011) following their kink bunching analysis in Denmark. Therefore,

¹¹Kleven and Waseem (2011) obtain larger but still quantitatively small elasticities of .05-.1 for the self-employed and .01-.04 for wage earners in the case of Pakistan where the jumps in average tax rates are much smaller, around 1%, and yet observable bunching is larger than in the case of the Danish tax scheme.

our result does not imply that the intensive elasticity of earnings with respect to the net-of-tax rate for highly paid foreigners is zero in all circumstances but rather that it is zero in this particular case.¹² In particular, this implies that firms and foreign workers are almost completely unable to game the system by manipulating earnings to take advantage of the scheme. Hence, we can conclude with great confidence that earnings manipulation or labor supply changes around the scheme notch does not affect our core migration effects that are the primary focus of our analysis.

Intensive Elasticity along Duration Margin

In order to analyze the intensive elasticity along the duration of stay margin, we exploit the fact that the scheme creates a kink in the individual life-time budget set as the scheme lasts for only 3 years. As we explained in the theory section, a number of individuals should stay exactly 3 years and hence create bunching in the density of stay durations at 3 years. To examine this, Figure 11 reports the density of duration of foreigners with (annualized) earnings above percentile 99.5th (P99.5-100) and hence above the eligibility threshold for the scheme and earnings between percentile 96 and percentile 99 (P96-99) and hence below the eligibility threshold for the scheme. Panel A is for years 1991-2005 (after the scheme was implemented) while Panel B is a control graph for years 1980-1990 (before the scheme was implemented). There is a clear spike at 3 years only in the upper group eligible for the scheme after the scheme was implemented providing very strong compelling evidence of an intensive response along the duration margin.

However, the spike is small relative to the large change in marginal incentives created the scheme. Figure 12 depicts the density of duration of foreigners with (annualized) earnings above percentile 99.5th for years 1991-2005, zooming around the 3-year cut-off date. The excess bunching at the kink represents about 2 month = 2/12 year of density. Let us denote by Δy^* this excess bunching (expressed in duration density equivalent terms). Using the methodology of Saez (2010), the intensive elasticity e^I can be estimated using the formula

$$e^I \simeq \frac{\log(1 + \Delta y^*/y^*)}{\log((1 - \tau_s)/(1 - \tau_d))} = \frac{\log(1 + 2/36)}{\log((1 - .25)/(1 - .55))} = .106$$

where $\tau_s = .25$ is the average tax rate on the scheme and $\tau_d = .55$ is the average tax rate on the

¹²Recent estimations of intensive elasticities of earnings among all workers in Denmark from Chetty et al. (2011) and Kleven and Schultz (2011) based on very compelling bunching evidence and tax reforms evidence (respectively) find positive but modest elasticities around 0.1 still much smaller than the migration elasticity we have found.

regular schedule. This formula applies for large changes in marginal incentives (as is the case for the scheme) for iso-elastic utilities with no income effects. Therefore, the intensive elasticity along the duration margin is also very small relative to the total elasticity.

4 Conclusion and Future Work

Our paper has analyzed the effects of income taxation on the international migration of top earners using the Danish tax scheme introduced in 1991 that offers preferential tax treatment to highly paid migrants to Denmark. This scheme offers a unique opportunity to credibly estimate elasticities of international mobility with respect to tax rates. Using population wide Danish administrative tax data, we have shown that the scheme doubled the number of highly paid foreigners in Denmark relative to slightly less paid ineligible foreigners, which translates into a very large elasticity of migration with respect to the net-of-tax rate around 1.5. We have shown that there is a significant but quantitatively very small response along the intensive earnings margin (work effort or earnings manipulation through tax avoidance). There is also evidence of sharp bunching of durations of stay of highly paid foreigners at the three year duration limit which translates into a significant but quantitatively small intensive elasticity of duration with respect to the net-of-tax rate. In the end, the migration elasticity is much more larger than the conventional within country elasticity of earnings with respect to the net-of-tax rate. As is the case for wealth mobility across countries (Kanbur and Keen, 1993), it is conceivable that elasticities are particularly large from small countries—such as Denmark—and that those small countries have the most to gain from preferential tax schemes for foreigners. Hence, “tax heavens” for highly skilled workers are likely to generate severe tax competition across European countries and severely limit the ability of European governments to use progressive taxation. This will require international coordination and the design of rules regulating such special schemes particularly in the European union.

Another important rationale put forward by Danish tax authorities as well as other European governments which have adopted similar preferential tax scheme for highly paid foreigners is that highly skilled workers generate positive externalities on their co-workers and the economy at large. The analysis of spillovers from human capital through knowledge diffusion has a long tradition in economics dating back to Marshall (1890) and has been used extensively in theoretical endogenous growth models. There is also substantial work in the urban economics

literature trying to uncover such spillover effects empirically (see e.g., Moretti, 2004). The Danish scheme creates a large and exogenous influx of highly skilled workers in the Danish economy, concentrated among specific industries. Therefore, in future work, we hope to make progress on estimating spillovers of scheme workers on co-workers and broader economic growth in two ways.

First, we could use a Bartik type instrument measure for the exposure of each industry or firm to the scheme. The instrument would be the fraction of highly paid foreign workers in the industry or individual firm just before the introduction of the scheme. This would allow us to measure the effect of attracting scheme workers on wage dynamics of domestic workers or non eligible foreigners as well as firms' hiring and growth behavior. Second, we plan to study specifically the researcher part of the scheme that we have not analyzed in this paper. We plan to analyze the effect of the influx of researchers in Denmark on the patents, publications, and placement of Ph.D. candidates of Danish research centers vs. comparable European countries such as Sweden. The comparison could be done across academic fields using the pre-scheme fraction of foreign researchers across fields.

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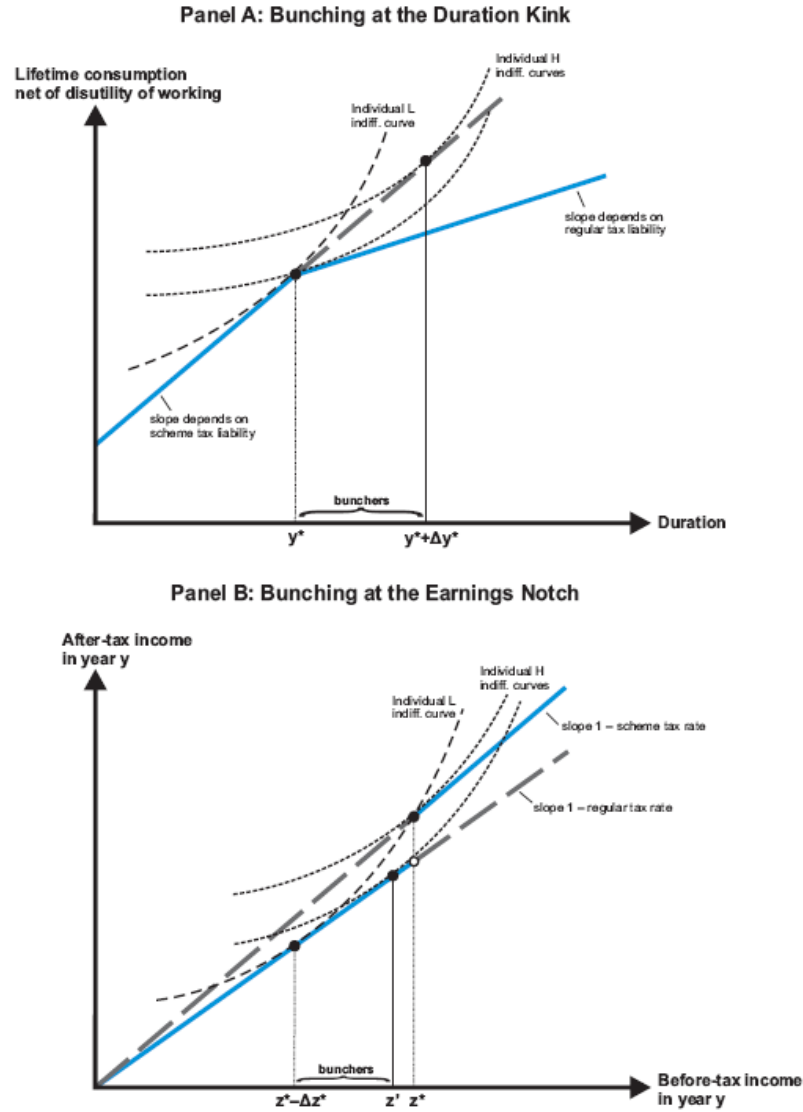


Figure 1: Duration and Earnings Responses to the Foreigner Tax Scheme in Denmark

The Figure illustrates behavioral responses to the duration kink (Panel A) and the earnings notch (Panel B). Panel A depicts the kink created by the scheme in the life-time budget defined over duration y of stay in Denmark and net consumption. The blue solid line budget represents the scheme where the slope is higher up to duration $y^*=3$ years and lower afterwards. The grey dashed line budget represents a scheme with indefinite duration. A scheme a finite duration generates bunching at the $y^*=3$ year duration as all individuals with preferences in between individual L and individual H bunch at the kink. Panel B depicts the notch created by the scheme at the eligibility earnings threshold z^* in the budget set of the individual (solid blue line). The notch creates piling up of individuals at the eligibility threshold and a corresponding gap in the density distribution just below the notch.

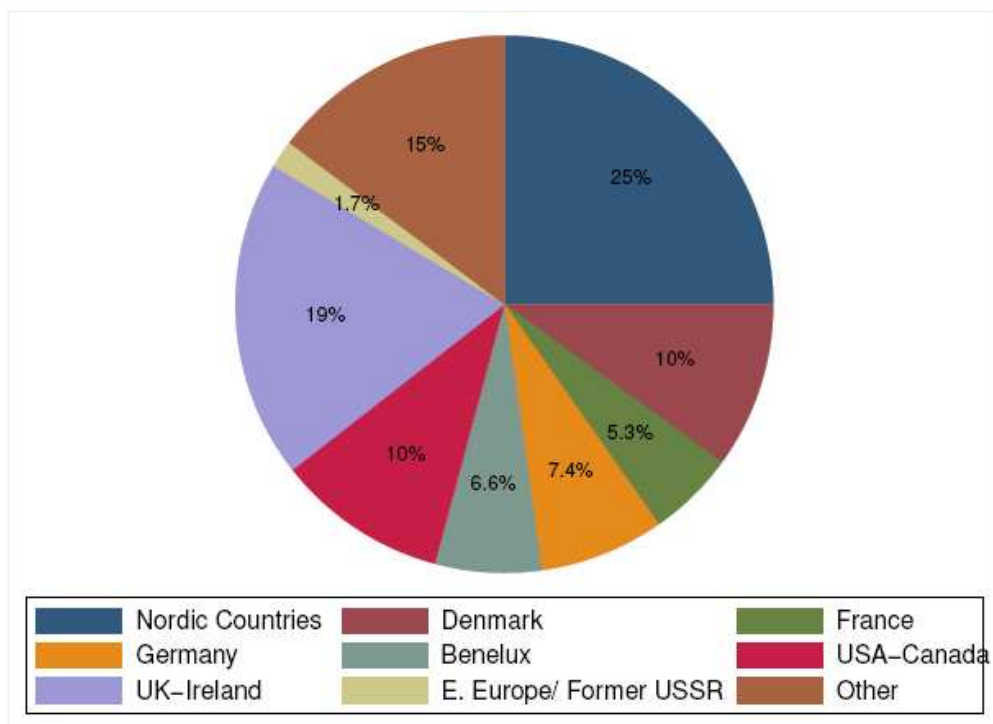


Figure 2: Citizenship of Scheme Beneficiaries, 1991-2006

The Figure reports the composition of beneficiaries of the tax scheme (excluding researchers) by country of citizenship (at the time of scheme) across all years 1991 to 2006.

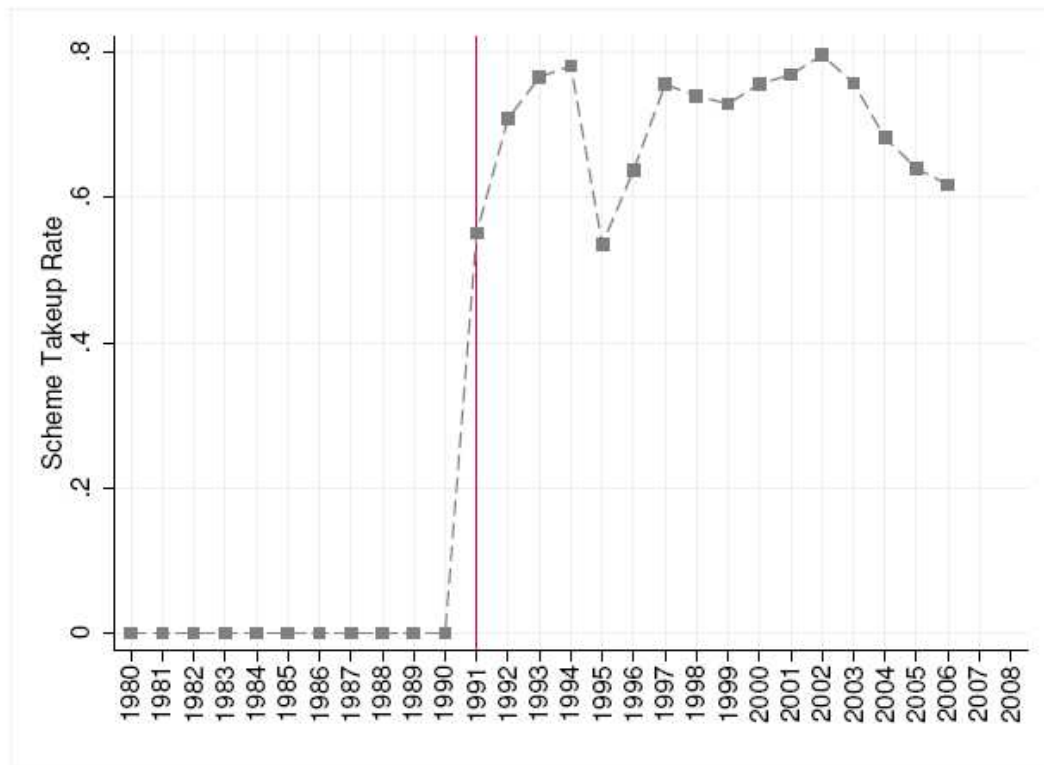


Figure 3: Scheme Take-up rate for eligible foreigners upon arrival

The Figure reports the take-up rate for the scheme among foreigners arriving in Denmark with (annualized) earnings above the eligibility threshold and who have not paid taxes in Denmark over the last 3 years.

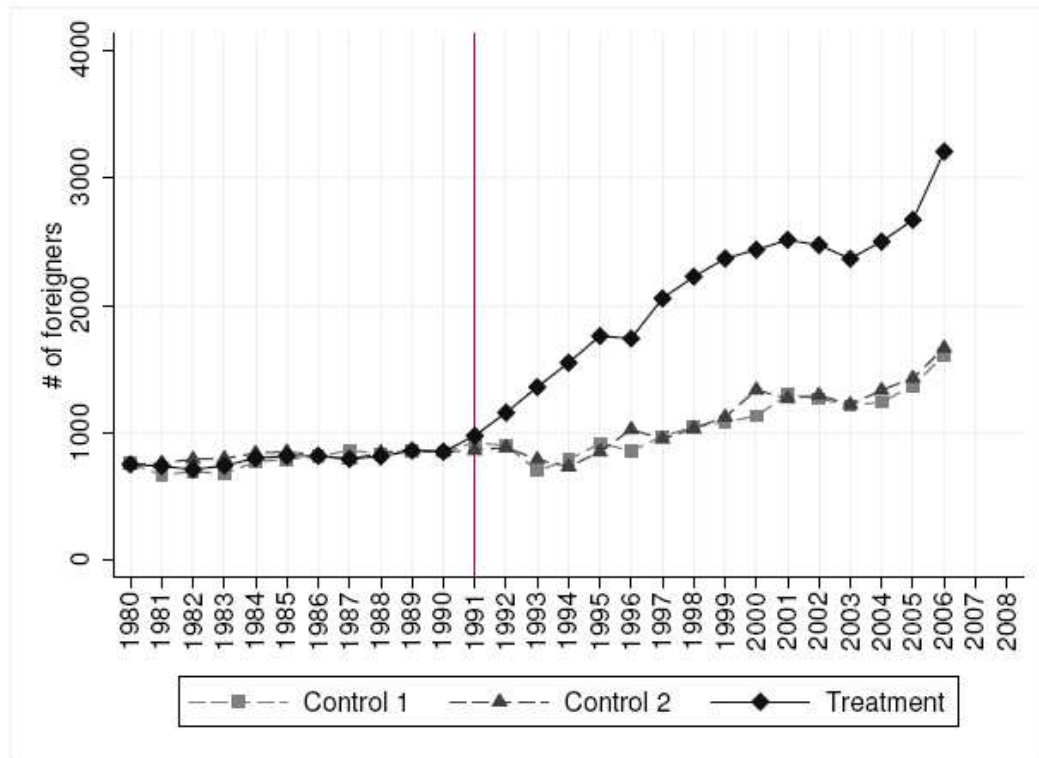


Figure 4: Total number of foreigners in different income groups

The Figure reports the number of foreigners with earnings above the scheme eligibility threshold (treatment series) from 1980 to 2006. As control groups, it reports the number of foreigners in Denmark with earnings between 80% and 90% of the threshold (control 1) and with earnings between 90% and 99.5% of the threshold (control 2). Both control series are normalized so that they match the treatment series in 1990—the year before the scheme was first implemented. The vertical line at year 1991 denotes the year the scheme was first implemented (the scheme was enacted in 1992 and applied retrospectively to all contracts starting after June 1991). All numbers are weighted by duration of stay during the year for part-year foreign residents and earnings are also annualized for part-year residents.

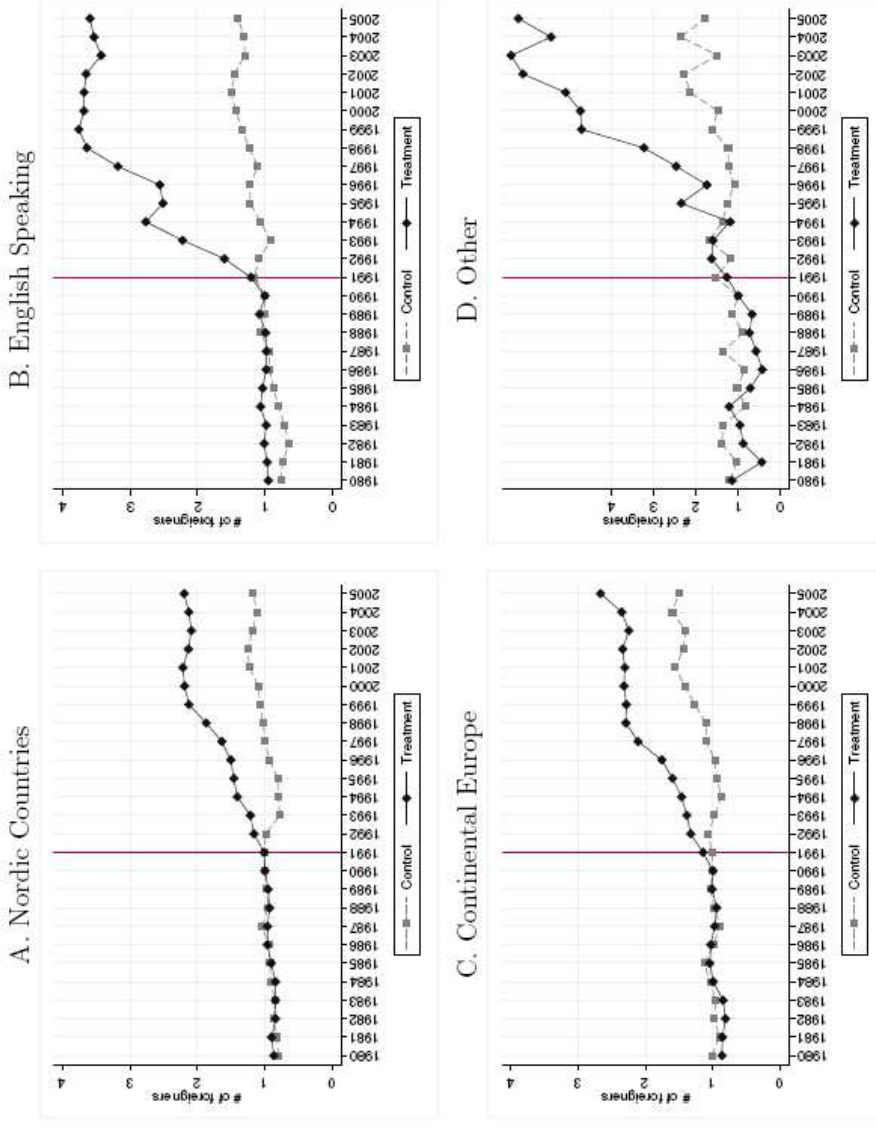


Figure 5: Total Number of foreigners in different income groups by regions of origin

The Figure reports total number of foreigners with earnings above the scheme eligibility threshold (treatment series) from 1980 to 2006 by country group of origin. As control groups, it reports the number of foreigners in Denmark with earnings between 80% and 99.5% of the threshold (control). All series are normalized to 1 in 1990—the year before the scheme was first implemented. The vertical line at year 1991 denotes the year the scheme was first implemented (the scheme was enacted in 1992 and applied retrospectively to all contracts starting after June 1991). All numbers are weighted by duration of stay during the year for part-year foreign residents and earnings are also annualized for part-year residents. Nordic Countries=Finland, Iceland, Norway, Sweden. English Speaking= Australia, Canada, Ireland, New Zealand, UK, US. Continental Europe= all other European countries excluding former communist Republics.

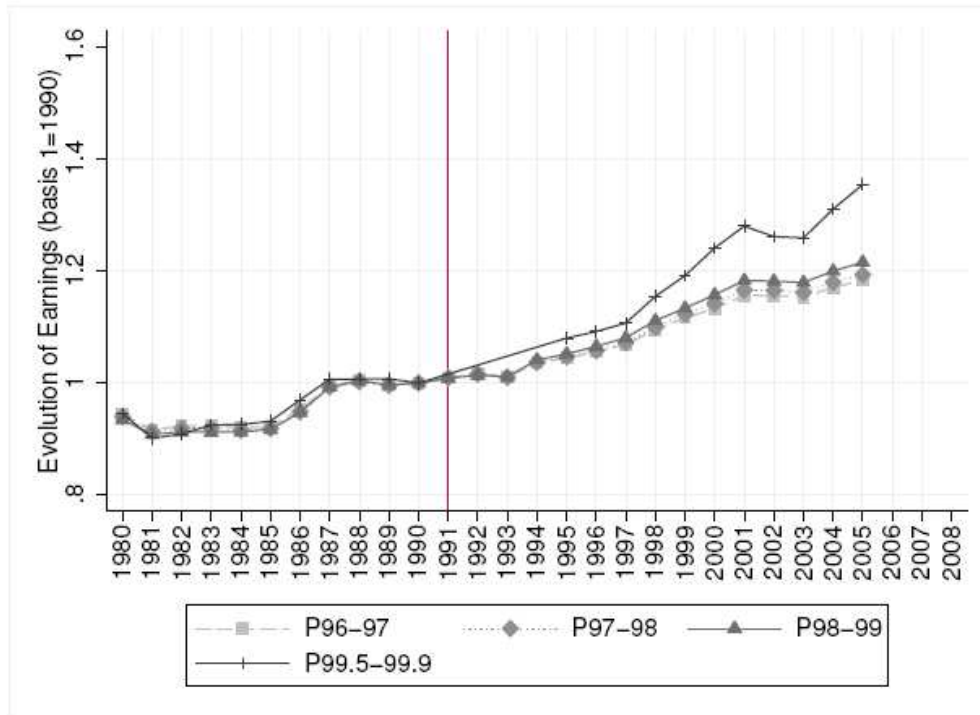


Figure 6: Average Earnings of Danish workers in different fractiles

The Figure plots average real earnings in various upper percentile groups both below and above the threshold among Danish citizens with positive earnings (normalized to 1 in 1990). P96-97 denotes all individuals between the 96th and 97th percentile, etc. The goal of this figure is to show that there were no widening of the upper part of the Danish earnings distribution during this period as all series grow at the same rate. The threshold for eligibility to the scheme is always between the 99th and the 99.5th percentile. The vertical line at year 1991 denotes the year the scheme was first implemented.

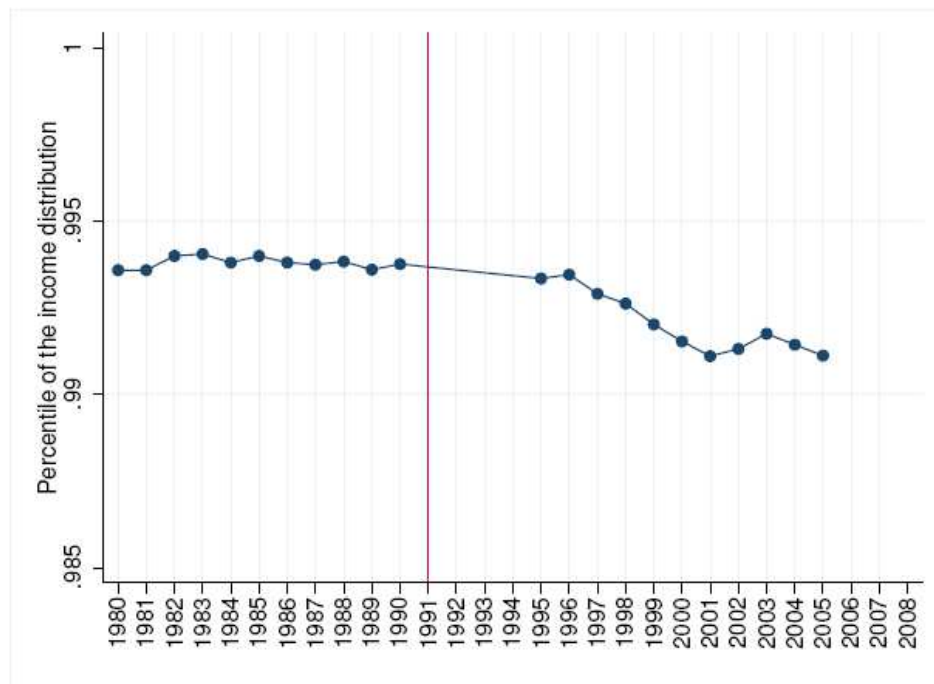


Figure 7: Evolution of the Level of the Scheme Income Threshold in the Income Distribution

The Figure reports the evolution of the level of the income threshold in the distribution of income in Denmark. The threshold for eligibility to the scheme always lies between the 99th and the 99.5th percentile, but as more foreigners enter the country with income above the threshold, the fraction of individuals with income above the eligibility threshold increases over time.

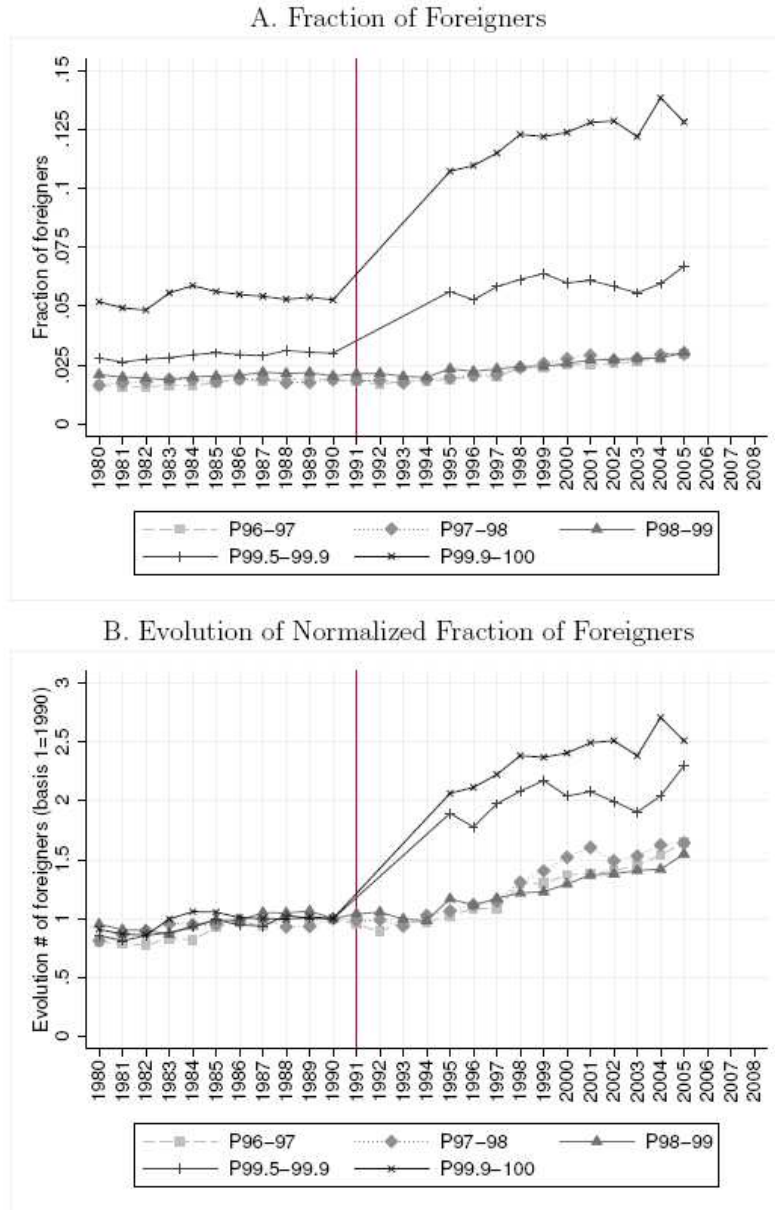


Figure 8: Total number of foreigners in different earnings groups

Panel A of the Figure plots the fraction of foreigners in various upper percentile groups of the distribution of earnings (including both Danish citizens and foreigners) from 1980 to 2005. The threshold for eligibility to the scheme is always between the 99th and the 99.5th percentile. P96-97 denotes all individuals between the 96th and 97th percentile, etc. There is a gap in 1991-1994 for the top two groups because the scheme related data does not provide scheme earnings for those years. Panel B repeats the same graph but normalizing the series to one in 1990 (just before the scheme introduction) to assess relative trends. The vertical line at year 1991 denotes the year the scheme was first implemented. Individuals are weighted by duration of stay for part-year residents.

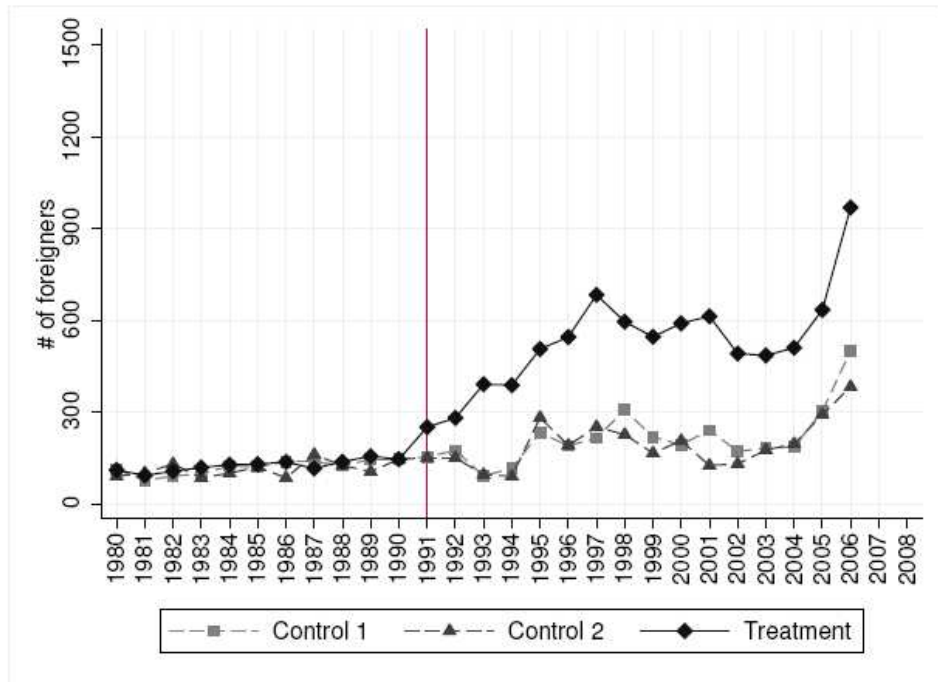


Figure 9: Total number of arrivals in different earnings groups

The Figure reports the number of foreigners with earnings above the scheme eligibility threshold (treatment series) arriving each year in Denmark from 1980 to 2006. As control groups, it reports the number of foreigners arriving in Denmark with earnings between 80% and 90% of the threshold (control 1) and with earnings between 90% and 99.5% of the threshold (control 2). Both control series are normalized so that they match the treatment series in 1990 the year before the scheme was first implemented. The vertical line at year 1991 denotes the year the scheme was first implemented.

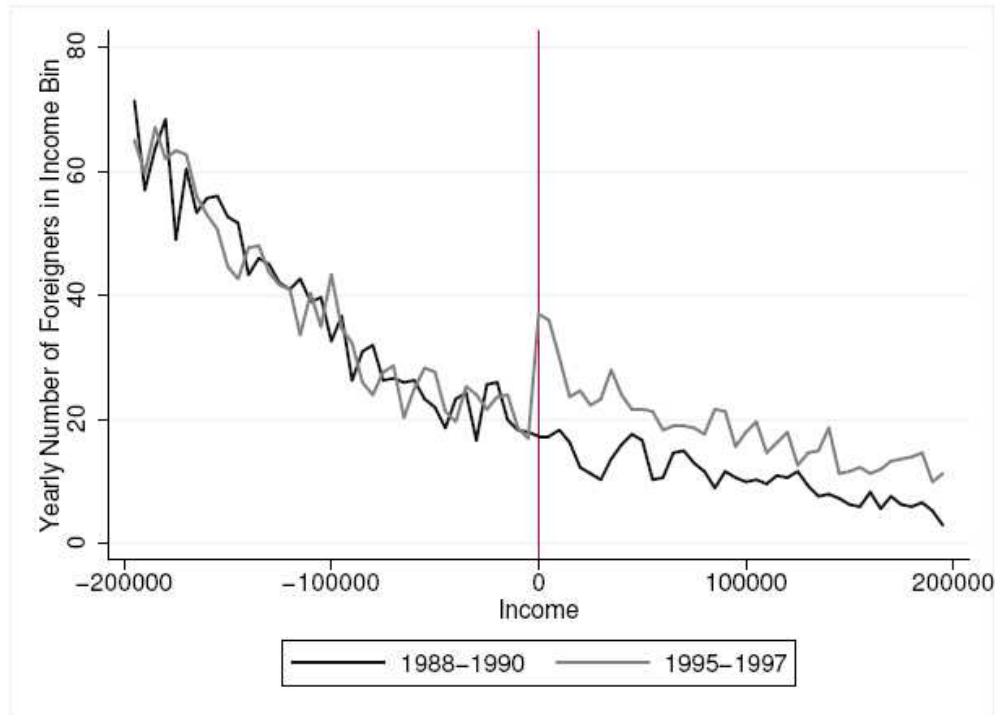


Figure 10: Earnings Density for Foreigners Before and After Scheme Introduction

The Figure reports the density of the earnings distribution of foreigners around the eligibility threshold (denoted by the vertical line) in 1995-7 (solid line after scheme implementation) and in 1988-90 (grey line before scheme implementation). For part year foreigners, earnings are annualized (i.e., divided by fraction of year present as scheme eligibility threshold is based on such annualized earnings). The graph shows that the scheme almost doubled the density above the threshold due to extensive migration responses and also created a small spike just at the eligibility threshold due to an intensive margin earnings response.

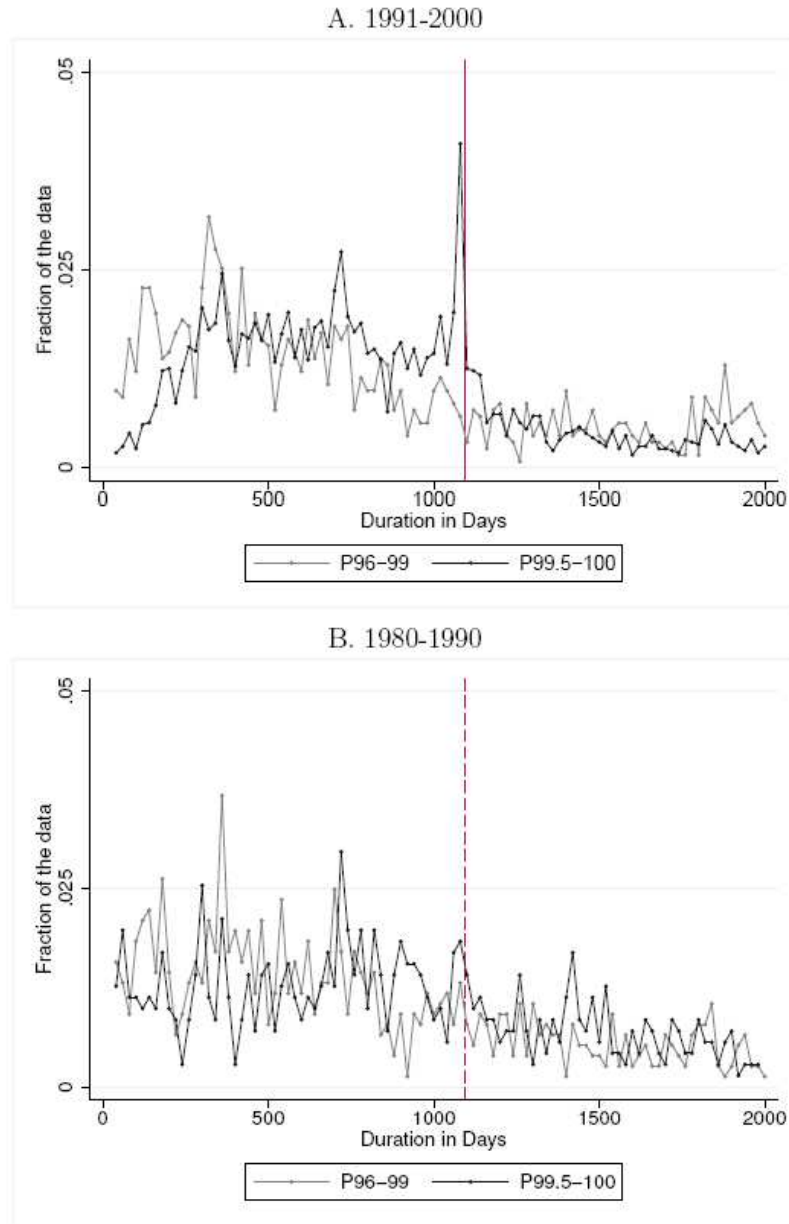


Figure 11: Duration of Stay Density for Foreigners

The Figure reports the density of duration of foreigners with (annualized) earnings above percentile 99.5th (P99.5-100) and hence above the eligibility threshold for the scheme and earnings between percentile 96 and percentile 99 (P96-99) and hence below the eligibility threshold for the scheme. Panel A is for years 1991-2000 (after the scheme was implemented) while Panel B is a control graph for years 1980-1990 (before the scheme was implemented). The vertical line denotes the 3-year threshold (maximum duration of the scheme). The spike at 3 year in Panel A (but not in Panel B) shows that the scheme has an impact on duration of stay along the intensive duration margin.

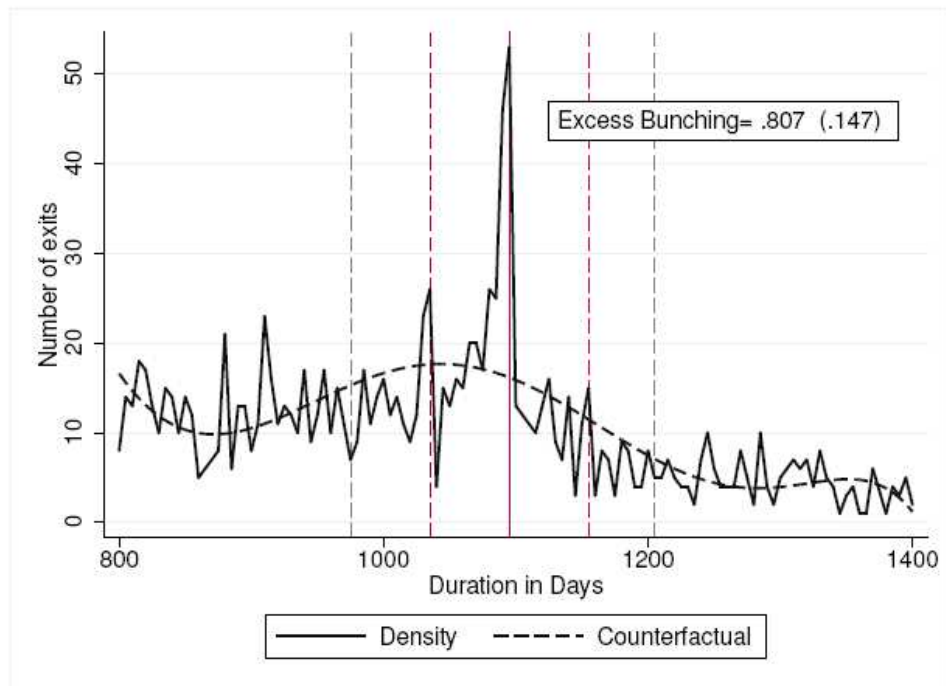


Figure 12: Excess Bunching at the 3-year Cut-off Duration

The Figure reports the density of duration of foreigners with (annualized) earnings above percentile 99.5th for years 1991-2005, zooming around the 3-year threshold (maximum duration of the scheme denoted in a vertical line). The spike at 3 year shows that the scheme creates excess bunching and hence that the scheme has an impact on duration of stay along the intensive duration margin.

Table 1: Difference-In-Difference Estimates of the Effect of the Tax Scheme on the Total Number of Foreigners

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	All Foreigners				Nordic Countries	Continental Europe	English Speaking Countries	Other
α_{tr*rf}	748.7*** (77.02)	421.8*** (53.44)	-30.7 (28.4)	706.5*** (48.6)	253.3*** (29.64)	202.1*** (20.87)	253.0*** (25.50)	40.30*** (10.13)
α_{tr}	-100.5* (43.05)	-100.5** (27.60)	-191.1*** (14.7)	177.44*** (27.19)	-17.19 (16.57)	-39.36** (11.67)	-32.31* (14.26)	-11.64* (5.662)
α_{rf}	194.8** (54.46)	24.27 (37.79)	17.77 (20.13)	102.94** (34.4)	34.03 (20.96)	49.04** (14.76)	82.66*** (18.03)	29.03*** (7.162)
α_0	734.1*** (30.44)	734.1*** (19.51)	462.62*** (10.39)	333.8*** (19.23)	290.3*** (11.72)	231.6*** (8.249)	174.9*** (10.08)	37.32*** (4.004)
$\frac{\alpha_{tr*rf}}{\alpha_{tr} + \alpha_{rf} + \alpha_0}$.90 (.16)	.65 (.11)	-.104 (.097)	1.11 (.16)	.825 (.16)	.84 (.15)	1.12 (.24)	.74 (.29)
Treatment	I > T			I > P99.5		I > T		
Control	.8*T < I < T			P96 < I < P99		.8*T < I < T		
Pre-reform	1980-1990	1980-1990	1980-1990	1980-1990	1980-1990	1980-1990	1980-1990	1980-1990
Post-reform	1995-2000	1991-1995	1991-1995	1991-1995	1995-2000	1995-2000	1995-2000	1995-2000
N	32	30	30	30	32	32	32	32

This table presents difference-in-difference estimates of the number of foreigners above the eligibility threshold (treatment) relative to foreigners between 80 and 100% of the threshold (control) before the scheme and after the scheme. The regression specification is $Y_{it} = \alpha_0 + \alpha_{tr} \cdot \mathbf{1}[i = 1] + \alpha_{rf} \cdot \mathbf{1}[t > 1991] + \alpha_{tr*rf} \cdot \mathbf{1}[i = 1] \cdot \mathbf{1}[t > 1991] + \nu_{it}$. The first row in the top panel presents the DD estimate α_{tr*rf} . The second panel presents the percent increase of foreigners due to the scheme, $\frac{\alpha_{tr*rf}}{\alpha_0 + \alpha_{rf} + \alpha_{tr}}$, where $\alpha_0 + \alpha_{rf} + \alpha_{tr}$ is the counterfactual number of foreigners that would have prevailed in the treatment group after 1991 in the absence of a reform. The bottom panel specifies the DD groups. Col. (3) is a placebo comparing two control groups.

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